able instance of self-fertilisation occurs in *R. abortivus*, whose petals do not close at night. It seeds profusely, yet is wholly neglected by insects, notwithstanding that it possesses large nectariferous glands. Instead of the flower closing, the slender pedicels droop at night, inverting the flower, and thus allowing the pollen to fall from the petals, on which it is shed, upon the stigmas. Mr. Meehan concludes that some deeper purpose than has yet been conceived governs the fertilisation of plants. In view of these examples, nature cannot "abhor" in-and-in-breeding, and it can hardly be that colour, fragrance, and honeyed secretion in flowers have been developed solely to secure cross-fertilisation. Evolutionists will await with interest further researches by Mr. Meehan, and confirmatory evidences from other inquirers.

THE BRITISH ASSOCIATION

REPORTS

Mr. Chrystal read the following summary of a Report upon a *Comparison of the B.A. Units of Electrical Resistance* that had been performed by himself and Mr. S. A. Saunder.—The experiments, of which I have here an account, were undertaken for the purpose of comparing the British Association Standards of Resistance now deposited in the Cavendish Laboratory at Cambridge. In the account of the work Mr. Saunder and myself have endeavoured as much as possible to enable anyone who consults it to judge by internal evidence of the accuracy of the comparison.

The experiments were so arranged as to give a check on their own accuracy.

In work of this kind the limit of accuracy is much sooner reached in the temperature than in the electrical measurements. It is to them therefore to which the greatest attention has to be given.

We took advantage of an extremely convenient source of nearly constant temperature in the tap-water of our experimenting room, which we found by careful observation to remain constant within the tenth of a degree centigrade for a sufficiently long time. By means of this we could find the differences between the resistances of the several coils at temperatures all near 10° C. The method used for obtaining these differences was a very convenient one, described by Prof. Carey Foster in the October number of the *Journal of the Society of Telegraph Engineers* for 1874.

To obtain the co-efficients of resistance-temperature variations it was necessary to make resistance measurements at a higher temperature. The temperature chosen was 16° C.

The coils were brought to this temperature by careful nursing for an hour or more.

The results of these experiments combined with those at the lower temperature gave the variation co-efficients. The differences at any given temperature could then be calculated.

Lastly, a series of direct comparisons were made, and the result compared with calculation in order to get an idea of the accuracy of our work.

There is a difficulty in giving a comparison between our results and those of the last measurements given in the British Association Report on Electrical Standards. This arises from a want of definite information about these last measurements.

Unfortunately on most of the coils the brass labels have never been completed as was intended, and although we think we managed to identify the coils described in the report with one exception, yet still more definite information is desirable. It is because we have felt this want that we have made our own report more minute than might otherwise have seemed necessary.

We hope that no ambiguity will exist when the coils are compared again either now to check our results or some ten years hereafter to find whether the standards have varied relatively to each other.

With this caution I give a series of temperatures at which the standards are equal to each other according to our measurements side by side with one of the temperatures given in the report.

		Last found.	B.A. Report.
Pt. Ir.	2	16·1	16·0
,,	3	15·8	15·8
Au. Ag.	58	15·3	15·3
Pt.	35	16·0	15·7
,,	36	15·8	15·7
Pt. Ag.	29	18·2	15·2

We have laid these measurements before the British Association in the hope that they will be found useful and be made accessible to those interested in such matters.

Report of the Committee for effecting the Determination of the Mechanical Equivalent of Heat.—Progress has been made with the experiments undertaken by Dr. Joule on behalf of the Committee. Friction of water is the method employed, and the average result of upwards of sixty experiments is 772·2 in British gravitation units at Manchester. The greatest deviation from the above average is $\frac{1}{200}$.

Experiments have yet to be made on the capacity for heat of the brass of which the calorimeter is constructed, which has provisionally been calculated from the results of Regnault for this alloy. The greatest possible error which may have arisen in this way is believed to be $\frac{1}{300}$th. Dr. Joule also proposes to compare his mercurial thermometers with the air thermometer with a view to obtain accurate boiling points, and thus correct values of the thermometric scale. The greatest correction which it may be found needful to apply on this account amounts to about $\frac{1}{400}$th. These maximum corrections, if taken in the same direction, would necessitate the addition or subtraction of 4·5 from the equivalent above named. The experiments made by Hirn on the friction of water have led him to the number 786; but the average of his results, derived from the friction, boring, and crushing of metals, gives 774.

Assuming that the above experiments and those made by Dr. Joule for the Committee on Standards of Electrical Resistance are to be relied on, the unit issued by it would appear to have a resistance one-fortieth too small. Inasmuch as the locality in which the experiments for that unit were conducted was open to objection, it appears desirable that they should be conducted under more favourable circumstances.

Report of the Committee on the Distribution of Erratic Boulders. Read by the Rev. H. W. Crosskey, M.A.—One hundred and sixty-five additional erratics have been catalogued west and south-west of Birmingham, of which 105 have travelled from considerable distances. West and south-west of the midland table-land a large proportion of the blocks are portions of highly indurated ash-beds. To the north and west granite is much more abundant.

Between the 400 and 500 ft. contour lines at Bothel (North Cumberland) is a large block which has been transported from the north-west portion of Dumfriesshire, about forty miles from N.N.W. to S.S.E. Fragments of Shap Fell Granite occur near Dufton (Westmoreland), 800 feet above the sea-level. The east and north-east boundary of the Arenig dispersion may be roughly defined as extending from Chirk by Cefn, Ruabon, Wrexham, Caergwile, Mold, and the east side of Halkin Mountain to Holywell, and thence in a westerly direction to the Vale of Clwyd. This line nearly coincides with the boundary of the great Northern Granite drift. The Welsh and the northern drifts have to a slight extent crossed the average boundary, and a few Arenig boulders have crossed the estuary of the Dee into the peninsula of Wirral, where they become mixed with the very abundant northern drift from the Lake district and the south of Scotland. The felspathic blocks from the Arenig range have radiated to great distances over an area extending from N.N.E. to E., and to short distances from east to south-east; and have found their way across valleys and over watersheds and high mountains. The direction of the glacial striæ on rock surfaces in the eastern part of North Wales as well as in the Arenig mountains, agrees in general with the course taken by the boulders.

The Committee invoke the assistance of geologists in carrying on their investigation. Schedules indicating the particulars required, may be had from the secretary. The rate at which the boulders are disappearing, owing to agricultural and building operations, makes it desirable to register their occcurrence without delay.

The report of the Close Time Committee gave an account of the steps which led to the passing of Mr. Chaplin's Bill for the Preservation of Wild Fowl last Session, and included a circular extensively distributed by the Committee to further that object. Lord Walsingham, Mr. Chaplin, and Mr. Rodwell were warmly thanked for their exertions in the matter. The Committee thought it possible that something further might be done to regulate the proceedings of bird-catchers; but the difficulties in the way appear so serious, that immediate success is not expected. The Sea Birds Preservation Act continues to work satis-

factorily on the whole, though there is reason to fear that its provisions have been disregarded in certain places. A few prosecutions in the coming year may be useful. The East Riding Justices have, with the assent of the Home Secretary extended the close time on the Yorkshire coast from August 1 to August 15.

Dr. M'Kendrick read the report of the committee *On Intestinal Secretion and Movement*. The conclusions to which the Committee had come were—First, that the application of various soda and potash salts to the intestinal mucous membrane produced a more or less profuse secretion, that caused by sulphate of magnesia, acetate of potash, sulphate of soda, and tartrate of potash and soda being most abundant; second, that the presence in the intestines or in the blood of atropia, morphia, chloral, &c., did not prevent the abstraction of sulphate of magnesia; third, that the splanchnic nerves were, as usually admitted, the vaso-motor nerves of the intestines, but either had no centrifugal fibres to their muscular coats or affected them only indirectly by diminishing their supply; fourth, the secretory nerves of the intestines had the small ganglia of the solar and superior mesenteric plexuses for their centres, and this secretion was unaffected by the splanchnics, the vagi, or the dorso-lumbar parts of the cord; fifth, destruction of the lumbar part of the cord after extirpation of the solar plexus produced hæmorrhage, or hyperaemia of the intestinal mucous membrane, which was absent after the division of the splanchnics, destruction of the semilunar ganglia and solar plexus, or division of the mesenteric nerves themselves; and sixth, the splanchnics were the afferent nerves for peristalsis of the intestines, the efferent stimulus probably reaching its intraparietal ganglia through the lumbar cord and the abdominal sympathetic, the effect of the former being inhibitory and the latter stimulating to these ganglia.

Mr. Heywood read the *Report of the Commiteee on the Metric System*. It pointed out that while the House of Commons had legalised the metric system for contracts and general purposes, no provision was made for the verification of the standards by the authorities, the consequence of which was that they could not be used in this country as they were liable to be seized. The Committee recommended that steps should be taken to have the weights and measures verified in the same manner as those of the imperial system. They regretted the striking out of the Education Code of the clause introduced by Mr. Forster referring to the metric system, and hoped it would be re-introduced. The report also entered into the question of the decimalisation of coinage.

The Report of the Commiitee on the Use of Steel for Structural Purposes, stated that after repeated correspondence with the Board of Trade, with the view of getting them to settle the conditions under which steel may be used, Colonel Yolland, R.E., Sir John Hawkshaw, F.R.S., and Mr. W. H. Barlow, F.R.S., had been appointed by the Board of Trade to endeavour to arrange these conditions.

Mr. Symons, secretary of the Rainfall Committee, read their Report to Section G. for the past year, from which it appeared that the rainfall of 1874 was slightly below the average, owing to a rather dry spring and exceedingly dry summer. The most remarkable feature of the year was the heavy fall of rain on October 6, when the average fall over England and Wales was slightly above 1 inch in the 24 hours, and the fall at most stations in North Wales and the Lake District was upwards of 5 inches. So heavy a fall over so large an area was rare. The rainfall of 1875 was greatly above the average in England (especially in the Midland Counties), and irregular in Scotland and Ireland. A very heavy rainfall occurred in Wales and Southern England on July 14, the fall in 24 hours exceeding 1 inch at 252 stations, 2 inches at 109, 3 inches at 39, 4 inches at 7, and 5 inches at 3 stations. The Committee reported last year the success of their efforts to improve the geographical distribution of rainfall stations in Ireland, showed that the gauges started at the cost of the Association had been supplemented by many others established at the cost of private individuals, and gave a map showing the present complete distribution of stations. Almost all the observers have proved good ones, and the returns had been forwarded with regularity. The period was too short to yield precise results, but a good system had been inaugurated and was in full operation. The Committee felt they had done service to rainfall work. When they commenced their labours, the weakest part of rainfall observations was the defective geographical distribution of the stations. This defect had now been materially lessened. By the grants of the Association nearly 250 gauges had been erected in districts hitherto without observations.

Mr. Bramwell asked what the Committee meant to do in the future.

Mr. Symons said he understood the Association wished to discontinue its grant to the Committee, and that the connection between the two should now cease. This he very much regretted, because if anything happened to himself he did not see how the work of the past could be maintained. Mr. Symons added that we had now in this country a system of observations which was the admiration of all countries. America and other countries were copying us. The system now embraced something like 2,000 stations, so scattered that it was scarcely possible to drop a man down in any place where he would be more than four or five miles from a rain-gauge. The consequence was, that when hydraulic and waterworks questions turned up, data were almost always available which did not exist ten years ago for ascertaining the quantity of water which could be collected from any given gathering ground. With reference to the future maintenance of the system it simply rested with himself.

It was ultimately stated by the President that the Sectional Committee considered the time had now arrived when this work should be taken up in a larger public spirit, and consequently that the grant hitherto made should now cease. This recommendation was made in the confident expectation that those who had hitherto so greatly benefited by the laborious and successful work carried on by Mr. J. S. Symons for the Association, would come forward and make the work of the Rainfall Committee their own. The Committee had also to record its most hearty thanks to that gentleman for his valuable services, which had proved so important to many branches of science, and had redounded to the credit of the British Association.

Mr. J. W. L. Glaisher gave the report of the *Committee on Mathematical Tables*. He stated that the whole of the theta function tables 0° to 89° (360 pp.) were now completed, and a copy taken from the stereotype plates was exhibited to the Section. He also gave an interim report of the Committee on mathematical notation and printing. The Committee had met and agreed to several suggestions with regard to notation, but it seemed desirable to postpone the report till next year, when the report on printing would be ready.

Mr. R. B. Hayward read the report of the Committee upon the improvement of geometrical teaching; it stated that the Committee approved generally of the syllabus issued by the Association for the improvement of geometrical teaching, although they criticised some few portions of it.

SECTION A.—MATHEMATICAL AND PHYSICAL.

One of the papers which excited the most attention was by Prof. Osborne Reynolds, *On the Resistance encountered by Vortex Rings, and the Relation between the Vortex Rings and the Stream-lines of a Disc*. It was illustrated by many most interesting experiments relating to the motion of vortex rings in a large trough of water. The following is an abstract of the paper:—

The comparatively small success which has attended nearly all attempts to refer the various movements of fluids to fundamental laws may, I think, be attributed principally to our being in ignorance of many most important circumstances of motion attending the phenomena with which we wish to deal.

We can see the way in which the surface of a fluid moves, but of the internal motions observation affords us no idea, we having no sense by which to perceive them. Accordingly, such steps as have been made towards success for the most part relate to surface phenomena, or to movements which have been rendered apparent by accident. My object, on the present occasion, is to describe certain results which have been obtained by colouring portions of the water within a tank so as to render them visible. These results are somewhat striking, and I venture to think that they are in some respects in advance of what has been hitherto taught; but they are now brought forward rather as illustrations of the importance of the method of study than on account of their own value.

The Cause of the Resistance to the Motion of Solids through Water not known.—The development of the theory of stream lines with which the name of the late Prof. Rankine is so intimately connected, has been a great advance, from the theoretical side, in the study of fluid motion. This theory, however, only applies

strictly to hypothetical fluids without viscosity, and its results as applied to water fall very far short of experimental verification.

Thus, as was so beautifully illustrated before this Section last year by Mr. Froude, a solid should move through a frictionless liquid in a rigid inclosure without resistance; the liquid moving out of its way, from front to rear, in filaments or streams which, closing together behind, cause pressure which exactly balances the pressure in front. In fact, however, water opposes very great resistance to the rapid motion of solids through it. If a ball which will just float be allowed to fall from a great height into water it will only descend a very short distance; and when we come to speeds like the speed of a shot, a foot of water opposes nearly as much resistance as an inch of iron. Opposite as these facts are to what the stream line theory might lead us to expect, they do not disprove the truth of this theory because it does not take into account the viscosity of water. But it is clear that before we can make much practical use of this theory in dealing with the only fluids with which we have to deal, we must ascertain in what way it is that viscosity affects the behaviour of these fluids, so that it may be taken into account in applying the theory. This is a point on which I think some light is thrown by rendering the motion of the water visible.

The Stream Line Theory applies to the Vortex Ring.—The idea of colouring the water to render its motion apparent was doubtless suggested by the effect of smoke and the beautiful phenomena of the smoke-ring. In the smoke-ring we have an instance of a most important form of fluid motion accidentally rendered invisible, of which we should otherwise have most certainly been in ignorance; as it is, however, it has caught the attention of mathematicians, and in the hands of Sir William Thomson, Prof. Tait, Helmholtz, and others, has led to most important researches.

That which is most striking in the smoke-ring is the regularity and extreme beauty of its internal structure. Our familiarity with objects moving rapidly through the air tends to diminish our surprise at the ease with which these rings move. But when we see these rings in water, this rapid motion and the small disturbance which they cause, although only a few inches below the surface, are, I think, the most striking points of the phenomenon.

Vortex rings in water were exhibited at Edinburgh, in 1871, by Mr. H. Deacon, but only on a very small scale, being formed of a single drop. About three years ago I tried a method of forming them, very similar to that used by Prof. Tait for smoke-rings. This method succeeded perfectly. From an orifice $\frac{3}{4}''$ in diameter, I could send rings the full length of my trough (20 feet), and with velocities so great, that during the first part of their course the eye could not follow them. It would appear, from the absence of all disturbance either behind the ring or at the surface, that these rings must move without resistance; and yet this appears at first sight to be inconsistent with the way in which the speed of the rings diminishes as they proceed, either in water or air. There is, however, a cause for this diminution of speed, which cannot properly be called resistance. The rings grow in size as they proceed, and consequently they are continually adding to their bulk water taken up from that which surrounds them, and with which this forward momentum has to be shared. A loss of velocity must result from this growth in size, and the only question with regard to resistance is whether the one of these is sufficient to account for the other, whether, notwithstanding the loss of velocity, the momentum of the moving mass remains constant.

To determine this I measured (by the best means I could devise) the momentum of a series of equal rings at different distances from this origin; the result was that (within the limits of accuracy of the experiments) there was the same momentum in the rings after they had travelled 15 feet, and were not moving more than 3 inches per second, as when at 2 feet from the origin, and moving more than 5 feet per second. I conclude, therefore, that these rings do move without any appreciable resistance.

When this freedom from resistance is considered along with the internal motion of the fluid in and around these rings, it shows that in them we have an instance, and I believe it is the only one, in which the stream line theory applies accurately to motion in a viscous fluid. The form of the mass of fluid moving forward is not nearly that of the ring, but is an oblate spheroid a good deal longer than this ring which it encloses.

This spheroid, like the ring, is continually growing, but at any instant it has a definite shape, and the motion of the water which surrounds it, is at that instant exactly the same as it would be according to the stream line theory if the spheroid were solid and the water were frictionless.

The spheroidal form of the bounding surface, which, of course being fluid, is perfectly flexible, is maintained against the unequally distributed pressure of the surrounding water by the motion of the water within it, the motion being such that at each point in the bounding surface it causes the same pressure as that arising from the motion of the external water.

The Effect of Viscosity on the Motion of the Ring.—The motion of the internal water, besides maintaining the shape of the bounding surface, is such that at each point of this surface the motion of the water in contact with it on the inside is identical with the motion of that in contact with the outside. So that not only is the bounding surface at each instant definite in form, but every point of the surface is in motion in exactly the same manner as the water in contact with it.

The action of the viscosity of the water in causing the gradual growth of the ring and its attendant mass is not confined to the bounding surface, but extends throughout the moving water both internal and external to this surface. There is a gradual diminution in the velocity with which the water moves along the stream lines from the centres outwards in all directions as far as the motion extends into the surrounding water, and, as is well known, when the velocity of a stream varies from point to point in a section across the direction of motion, the effect of viscosity is towards equalising the velocity. Hence, in the case of the vortex ring, the effect of viscosity will be to diffuse the motion outwards, to diminish the whirling velocity at the centre where greatest, and to extend the space through which the water is in motion, that is, to diminish the velocity and extend the size of each element of the ring.

And it appears that this effect to diminish the velocity and extend the size of the ring is the only effect of viscosity. That is to say, if the water at any instant were to lose its viscosity, then the ring would proceed onwards in exactly the same manner as it was proceeding at that instant, for the internal motion would be just as necessary to balance the external pressures and preserve the form of the bounding surface in frictionless fluid, as in water, and hence the same law must hold between the internal and external motions.

The author then supposes that at a certain instant one of the vortex rings is converted into ice, and further, that there is no friction between the surface of the ice and the surrounding water. He proceeds:—

Whatever might be the resistance of the ideal smooth ice, it is clear that its actual resistance must exceed this by what shipbuilders call its skin resistance, the drag of the water moving past the surface. This may be estimated from the resistance of a plane surface of equal extent when moving edgewise through the water, and this is not much.

This, however, is only one of the effects of the surface friction. Whatever drag the friction may cause on the surface, there is an equal drag on the water moving past it, and thus the surface friction aids the diffusion in diminishing the velocity with which the water would otherwise move in the stream lines near the surface, and so tends to increase the disturbance of the stream lines in the rear of the solid.

Actually we find that the resistance resulting from the disturbance of the stream lines is ten times as great as the mere skin resistance, and so far is a solid, of the shape of the bounding surface of the vortex ring, from moving freely, that if it be set in motion, it stops at once and is altogether dead in the water.

That the disturbance of the stream lines as described above really takes place, is shown when we colour the water. When we first start the solid we see a somewhat irregular vortex ring behind it, which grows rapidly and then breaks up; after this the water behind is all confused, and follows the solid. Whereas with the vortex ring, if there are streaks of colour in the water through which the ring passes, it leaves them so nearly as they were before, that there is scarcely a trace of its path. Thus this disturbance of the stream line appears to be the cause of the resistance encountered by a solid over and above the skin-resistance.

The magnitude of the effect depends on the curvature of the streams, and hence we see why a body having a fine after-part like a fish encounters so much less resistance than a full body like a spheroid. Whereas if the stream lines were complete according to the theory, the extra surface of the fish should cause it much greater resistance.

Relation between the Vortex Ring and the Stream Lines of a Disc.—Another matter on which I have been able to throw some light by colouring the water, relates to the form of the stream lines of a thin surface, such as a disc. It is, I believe, generally assumed that the theory of stream lines shows all bodies would

move without resistance in a perfect fluid; and that thin surfaces are no exception to the general rule, but I am not aware that any satisfactory figure for the stream line of such bodies has as yet been given.

Now what I have to show raises two very important points in connection with the stream-line theory, even as applied to a perfect fluid. In the first place it will appear that a thin open surface has no stream lines of its own (so to speak) except such as it can claim as forming part of a closed surface. And in the second place it will appear that the closed surface may assume the form of a cylinder of indefinite length continually passing away from the thin surface, in which case the surface or vane does not move freely even through a frictionless fluid. If we place a disc in front of one of these rings, the ring comes on until the disc is against the bounding surface, and then carries the disc on with it. It is certainly surprising to see a flat disc moving freely through the water. I doubt not that the general impression is that a thin flat disc is about the worst form of body to move through water. And so it is, except when it has a vortex ring behind it.

Owing to the growth of the ring, if the speed of the disc be maintained, the ring will gradually fall behind the disc, and the disturbance caused in front will break it up. But if the disc be allowed to move with the ring it will move freely as far as the ring goes. A disc when first started forms its own ring. Thus if a disc be floated on a light bar of wood, when the wood is drawn forward at first, the disc offers considerable resistance to its motion, but this resistance soon dies away, and if then the bar be released, the disc will proceed steadily onward with a gradually diminishing velocity.

A little colour in the water shows how the ring is formed and how it moves onward behind the disc.

The Resistance of an Inclined Vane.—The fact that the disc will start its own ring, will close its own surface, is due to its being symmetrical with respect to this surface. Half a disc will not do it, much less any portion of the spheroid which was inclined to the front.

When we draw a disc or flat surface edgewise through the water it causes a continuous vortex cylinder which, forming at the forward point of the vane passes away behind. The gyratory motion of the water is somewhat disturbed by the friction of the vane sliding past it, but by letting a little air down with the vane the central lines of the vortices may be shown for several feet in length.

Having to form the vortices the forward edge encounters the greatest resistance, and the whole resistance is steady and continuous.

If my reasoning is right these facts are somewhat at variance with the general notion as regards the results of the stream-line theory, and at all events they furnish definite ideas of the results we have to explain. It is, however, with the utmost diffidence that I venture to bring forward my own explanation before such an authoritative body as Section A. in the University of Glasgow, and my chief object has been to illustrate the method of studying fluid motion by observations on the motion of partly coloured water.

On the Protection of Buildings from Lightning, by Prof. J. Clerk Maxwell.—Most of those who have given directions for the construction of lightning-conductors have paid great attention to the upper and lower extremities of the conductor. They recommend that the upper extremity of the conductor should extend somewhat above the highest part of the building to be protected, and that it should terminate in a sharp point, and that the lower extremity should be carried as far as possible into the conducting strata of the ground, so as to "make" what telegraph engineers call "a good earth."

The electrical effect of such an arrangement is to *tap*, as it were, the gathering charge by facilitating a quiet discharge between the atmospheric accumulation and the earth. The erection of the conductor will cause a somewhat greater number of discharges to occur at the place than would have occurred if it had not been erected; but each of these discharges will be smaller than those which would have occurred without the conductor. It is probable, also, that fewer discharges will occur in the region surrounding the conductor.

It appears to me that these arrangements are calculated rather for the benefit of the surrounding country and for the relief of clouds labouring under an accumulation of electricity, than for the protection of the building on which the conductor is erected.

What we really wish is to prevent the possibility of an electric discharge taking place within a certain region, say in the inside of a gunpowder manufactory. If this is clearly laid down as our object, the method of securing it is equally clear.

An electric discharge cannot occur between two bodies, unless the difference of their potentials is sufficiently great, compared with the distance between them. If, therefore, we can keep the potentials of all bodies within a certain region equal, or nearly equal, no discharge will take place between them. We may secure this by connecting all these bodies by means of good conductors, such as copper wire ropes, but it is not necessary to do so, for it may be shown by experiment that if every part of the surface surrounding a certain region is at the same potential, every point within that region must be at the same potential, provided no charged body is placed within the region.

It would, therefore, be sufficient to surround our powder-mill with a conducting material, to sheathe its roof, walls, and ground-floor with thick sheet-copper, and then no electrical effect could occur within it on account of any thunderstorm outside. There would be no need of any earth connection. We might even place a layer of asphalte between the copper floor and the ground, so as to insulate the building. If the mill were then struck with lightning, it would remain charged for some time, and a person standing on the ground outside and touching the wall might receive a shock, but no electrical effect would be perceived inside, even on the most delicate electrometer. The potential of everything inside with respect to the earth would be suddenly raised or lowered as the case might be, but electric potential is not a physical condition, but only a mathematical conception, so that no physical effect would be perceived.

It is therefore not necessary to connect large masses of metal such as engines, tanks, &c., to the walls, if they are entirely within the building. If, however, any conductor, such as a telegraph wire or a metallic supply-pipe for water or gas comes into the building from without, the potential of this conductor may be different from that of the building, unless it is connected with the conducting-shell of the building. Hence the water or gas supply pipes, if any enter the building, must be connected to the system of lightning conductors, and since to connect a telegraph wire with the conductor would render the telegraph useless, no telegraph from without should be allowed to enter a powder-mill, though there may be electric bells and other telegraphic apparatus entirely within the building.

I have supposed the powder-mill to be entirely sheathed in thick sheet copper. This, however, is by no means necessary in order to prevent any sensible electrical effect taking place within it, supposing it struck by lightning. It is quite sufficient to inclose the building with a network of a good conducting substance. For instance, if a copper wire, say No. 4, B.W.G. (0·238 inches diameter), were carried round the foundation of the house, up each of the corners and gables and along the ridges, this would probably be a sufficient protection for an ordinary building against any thunderstorm in this climate. The copper wire may be built into the wall to prevent theft, but should be connected to any outside metal such as lead or zinc on the roof, and to metal rain-water pipes. In the case of a powder-mill it might be advisable to make the network closer by carrying one or two additional wires over the roof and down the walls to the wire at the foundation. If there are water or gas-pipes which enter the building from without, these must be connected with the system of conducting-wires, but if there are no such metallic connections with distant points, it is not necessary to take any pains to facilitate the escape of the electricity into the earth.

Still less is it advisable to erect a tall conductor with a sharp point in order to relieve the thunder-clouds of their charge.

It is hardly necessary to add, that it is not advisable, during a thunderstorm, to stand on the roof of a house so protected, or to stand on the ground outside and lean against the wall.

On a Cyclone Periodicity, in connection with Sun-spot Periodicity, by C. Meldrum.—This paper is a continuation of the one published in the report for 1874; it contains a discussion of the cyclones that occurred in the Indian Ocean, from the equator to 32° S. and 0° to 120° E., in the years 1868-75. From 1868 to 1872 the cyclonic area increased, and since 1872 it has been decreasing. In 1868 it was two millions of square miles, in 1872 between four and five millions, and in 1875 nearly two millions. The rainfall over the globe generally seems to have had a similar march, the rainiest year being 1872.

Mr. O. J. Lodge exhibited diagrams of a model to illustrate mechanically the passage of electricity through metals, electrolytes, and dielectrics, according to Maxwell's theory. The model consisted of an endless cord passing with friction through buttons supported on elastic strings, and by altering the relation

between the friction and the elasticity of different parts it could be made to exhibit very completely the phenomena observed when an electromotive force is made to act (1) between the ends of a metal wire; (2) through an electrolytic liquid; (3) in an accumulator with perfectly insulating dielectric; (4) across a dielectric which is homogeneous, but has a slight conducting power; (5) across a non-homogeneous or stratified dielectric, in which a "residual charge" is possible. To illustrate in a simple manner the phenomena observed in a submarine cable, the cord might be made elastic.

Capt. A. W. Baird, R.E., contributed a paper *On Tidal Operations in the Gulf of Cutch*.—The primary object of the operations was to determine whether secular changes in the level of the land at the head of the gulf, *i.e.*, the "Runn of Cutch," are taking place. Col. Walker at first intended to restrict the observations to a few weeks' duration, but he found that by extending them to a period of a little over a year, scientific results of the highest value would be obtained, and also that this course would be necessary in order to obtain data sufficient to detect minute changes in the relative level of land and sea. The author described the difficulties that had been experienced; but stated that the whole of the tidal and meteorological observations were in progress of reduction, and when completed were likely to afford results of importance. It was hoped that the effect of the wind and barometer upon the tide might be determined more accurately than had yet been done. Tracings of the actual diagrams were exhibited, and the tidal curves were seen to be very regular and continuous.

The number of important experiments shown before Section A at this meeting was very remarkable. Besides the experiments of Prof. Osborne Reynolds already referred to, there were several others relating to liquids. Prof. James Thomson illustrated experimentally the origin of the windings of rivers in alluvial plains, as explained in a recent paper in the Royal Society's *Proceedings*, and Sir William Thomson showed many experiments upon the precessional motion of a spheroidal top filled with liquid. Sir William Thomson also exhibited a new form of astronomical clock with a free pendulum actuated by an independent governor to give approximately correct uniform motion to the escapement-wheel. Mr. H. W. Bosanquet illustrated experimentally his paper on the conditions of the transformation of pendulum vibrations, and Mr. Colin Brown exhibited his voice harmonium in connection with his paper on just intonation. Sir William Thomson explained a method of taking deep-sea soundings in a ship moving at high speed by means of pianoforte-wire and an apparatus which was exhibited and explained. The Rev. J. Ker described an experiment proving rotation of the plane of polarisation of light reflected from a magnetic pole. The attendance throughout was excellent, and the room was generally crowded; in fact in recent times there has been no meeting at which so much interest has been taken in the proceedings of this Section. Owing to the number of papers the Section was divided into two on the Monday and Tuesday, and the Section met to finish the work on Wednesday. Saturday was, as usual, devoted to mathematics. There were altogether twenty-three mathematical papers, among which may be mentioned those by Prof. Cremona on systems of spheres and systems of lines, by Prof. Tait on two general theorems relating to closed curves, by Mr. J. W. L. Glaisher, giving determinants expressing the number of partitions of a number, and the sum of the divisors of a number; by Prof. Jung, of Milan, on the inverse problems of the moments of inertia and the moments of resistance of a plane figure, and on a construction for the central nucleus (*Centralkern*, of Culmann) of a body, and by Mr. G. H. Darwin on graphical interpolation and integration. Two new committees were appointed at the recommendation of Section A, one for commencing experiments upon the elasticity of wires, and the other upon the lunar disturbance of gravity.

SECTION C.—Geology.

On the Upper Limit of the essentially Marine Beds of the Carboniferous System in the British Isles, and the necessity for the establishment of a Middle Carboniferous Group, by Prof. E. Hull, M.A., F.R.A.S.—Prof. Hull distinguished seven stages in the Carboniferous Rocks, each stage being capable of identification by its fossils over large areas in Great Britain and Ireland. These stages are:—

(1) Upper Coal Measures.
(2) Middle Coal Measures.
(3) Lower Coal Measures or Gannister Beds.
(4) Millstone Grit.
(5) Yoredale Rocks.
(6) Carboniferous Limestone.
(7) Lower Limestone Shale.

He argued that as thirty-three out of fifty-three species of marine shells pass from the Carboniferous Limestone upwards into the Gannister Beds, while only five passed up into the Middle Coal Measures, a palæontological break was indicated of such magnitude as to warrant a more distinct separation of the Gannister Beds from the Middle Coal Measures; especially as the shells of the former are marine, while many palæontologists regard those of the latter as of fresh-water origin. He therefore proposed to include all from the Gannister Beds to the Yoredale Rocks as Middle Carboniferous; the term Lower Carboniferous to include as at present the Carboniferous Limestone and Lower Limestone Shale.

Note on Sections exhibiting Variation of thickness in the Middle Coal Measures of West Lancashire, by C. E. de Rance, F.G.S.—The sections described by the author lie between Prescot and Barnsley, where the Middle Coal Measures, containing several thick coal-seams, and the Gannister Beds, containing few important seams, are represented. Having made many sections of the Middle Coal Measures of the district, Mr. de Rance was satisfied that the amount of the subsidence from south to north for a distance of ten miles, increased at the rate of about 60 feet per mile, and that the deposition of the Coal Measure strata kept pace with the subsidence.

On the Changes affecting the Southern Extension of the Lowest Carboniferous Rocks, by G. A. Lebour, F.G.S.—The author contended for the division of the Carboniferous Rocks into Upper and Lower, drawing the line between the Millstone Grit and Yoredale Series. Dealing with the lower division, it was pointed out that the Upper Old Red Sandstone was in part identical with, or passed upward into Macharen's "Calciferous Sandstone," known in the North of England as "Tuedian," and in Ireland as "Valentian." In England the upper limit of the "Tuedian" is equally indefinite, as the series dovetails into the lower members of the "Bernician" Group, in which term the author includes the "Yoredale Series and Calcareous Group in part, Scar Limestone Series and Calcareous Group in part, *plus* Carbonaceous Group in part."

On the Mountain Limestone on the West Coast of Sumatra, by Prof. Ferdinand Roemer, of Breslau.—Dr. Verbeck, Director of the Dutch Geological Survey of Sumatra, sent to the author a large collection of fossils from the west coast, for determination and description. The result of this investigation is that the Mountain Limestone is developed on the western coast of Sumatra, with mineralogical and palæontological characters, very similar to those of the same formation in Europe. The same genera of shells, and partly the same species occur in these widely remote localities. Hitherto the Mountain Limestone has only been known to occur in the Malay Archipelago in Timor, a small but characteristic Mountain Limestone fauna from that island having been described by Prof. Beyrich. It may be expected that by-and-by the Mountain Limestone may be ascertained to have a wider range in the Malay Archipelago than is at present known. From the geographical position of Java, between Sumatra and Timor, it is probable that a zone of Mountain Limestone, and perhaps of other palæozoic rocks, may be found to exist there, although partly hidden by volcanic and tertiary deposits.

On some New Minerals and on Doubly-refracting Garnets, by Prof. A. von Lasaulx, of Breslau.—Prof. von Lasaulx exhibited specimens of a new mineral from Girgenti, Sicily, where it occurs in small cubes on crystals of sulphur and celestine. Its chemical composition is,—silica 86 per cent., water 3 per cent., iron and strontium small quantities, and sulphuric acid, or some acid of the thionic series not yet determined, 7 per cent. From the behaviour of the mineral before the blowpipe, the author named it melanophlogite. The author also described a series of garnets exhibiting the phenomena of double refraction.

On the Raised Beaches of the Cumberland Coast between Whitehaven and Boness, by A. Russell, C.E., F.G.S., and T. V. Holmes, F.G.S.—The authors exhibited a map showing several fragments traced by themselves, of raised beaches sloping inland from 25 ft. above the present sea-level to an upper limit of 40 ft. The terraces are covered by low gravel ridges parallel to the old

cliff line, similar to the little mounds left by the sea at the present day half-way between the mark of the highest spring tides and that of the lowest neap tides. With regard to the date of the elevation of the beaches, the authors observed that all the evidence available tended to place it before the time of the Roman occupation.

Tidal Retardation—Argument for the Age of the Earth.—The Secretary read a paper by James Croll, LL.D., F.R.S., of the Geological Survey of Scotland, *On the Tidal Retardation Argument for the Age of the Earth.* Many years ago Sir William Thomson demonstrated from physical considerations that the views which then prevailed in regard to geological time and the age of our globe were perfectly erroneous. His two main arguments were—first, that based on the sun's possible age; and secondly, that based on the secular cooling of the earth. More recently he has advanced a third argument (*Trans.* Geol. Soc. of Glasgow, vol. iii., p. 1), based on tidal retardation. It is well known that owing to tidal retardation the rate of the earth's rotation is slowly diminishing, and it is therefore evident that if we go back for many millions of years we reach a period when the earth must have been rotating much faster than now. Sir William's argument is, that had the earth solidified several hundred millions of years ago the flattening at the poles and the bulging at the equator would have been much greater than we find them to be. Therefore, because the earth is so little flattened it must have been rotating, when it became solid, at very nearly the same rate as at present. And as the rate of rotation is becoming slower and slower, it cannot be so many millions of years back since solidification took place. A few years ago I ventured to point out (NATURE, August 21, 1871; "Climate and Time," p. 335) what appeared to be a very obvious objection to the argument, and as the validity of the objection, as far as I am aware, has never been questioned, I have been induced to believe that the argument referred to had been abandoned. But I find that Prof. Tait in his work on "Recent Advances in Physical Science," restates the argument as perfectly conclusive, and makes no reference whatever to my objection. As the subject is one of very considerable importance, I may be permitted to direct attention to the objection in question, which briefly is as follows:—

It has been proved by a method pointed out a few years ago (*Philosophical Magazine*, May, 1868, pp. 378–384, February, 1867, p. 130, "Climate and Time," Chap. xx. *Transactions* of Geological Society of Glasgow, vol. iii., p, 153), and which is now generally admitted to be reliable, that the rocky surface of our globe is being lowered, on an average, by subærial denudation at the rate of about 1 foot in 6,000 years. It follows as a consequence from the loss of centrifugal force resulting from the retardation of the earth's rotation, occasioned by the friction of the tidal wave, that the sea-level must be slowly sinking at the equator and rising at the poles. This, of course, tends to protect the polar regions, and expose equatorial regions to subærial denudation. Now it is perfectly obvious that unless the sea-level at the equator has, in consequence of tidal retardation, been sinking during past ages at a greater rate than 1 foot in 6,000 years, it is physically impossible the form of our globe could have been very much different from what it is at present, whatever may have been its form when it consolidated, because subærial denudation would have lowered the equator as rapidly as the sea sank. But in equatorial regions the rate of denudation is, no doubt, much greater than in the temperate regions. It has been shown in the papers above referred to, that the rate at which a country is being lowered by subærial denudation is mainly determined not so much by the character of its rocks as by the sedimentary carrying power of its river systems. Consequently, other things being equal, the greater the rain-fall the greater will be the rate of denudation. We know that the basin of the Ganges, for example, is being lowered by denudation at the rate of about 1 foot in 2,300 years, and this is probably not very far from the average rate at which the equatorial regions are being denuded. It is therefore evident that sub-ærial denudation is lowering the equator as rapidly as the sea-level is sinking from loss of rotation, and that consequently we cannot infer from the present form of our globe what was its form when it solidified. In as far as tidal retardation can show to the contrary, its form may have been as oblate as that of the plant Jupiter when solidification took place.

There is another circumstance which must be taken into account. The lowering of the equator by the transference of materials from the equator to the higher latitudes must tend to increase the rate of rotation, or, more properly, it must tend to lessen the rate of tidal retardation.

On Siliceous Sponges from Carboniferous Limestone near Glasgow, by John Young, F.G.S.—Mr. Young observed that siliceous sponges had not hitherto been obtained from deposits of Carboniferous Limestone age in Britain. Recently, however, Mr. John Smith had discovered large numbers of them in fissures in a limestone at Cunningham Baidland, near Dalry, Ayrshire. The limestone bed in which they occur is 40 feet thick, and belongs to the upper division of the Carboniferous Limestone series. It contains, at different horizons, producti and spirifers, corals, crinoids, and polyzoa. Prof. Young and the author proposed to name them *Acanthaspongia Smithii.*

On the Granite of Strath-Errick, Loch Ness, by James Bryce, LL.D.—Having ascertained that the gold of Sutherland occurred not in quartz veins, but in the granite itself, Dr. Bryce tested the granite of Strath-Errick, and was rewarded by finding gold there also, although in small quantities. Proceeding to examine more carefully than had previously been done the relations of this granitic mass to the surrounding rocks, he found that, although at one locality it clearly overlaid the Lower Old Red Sandstone, in another place it alternated with slate, as if the slate had been brought up by the granite. It was remarkable that, although the slates are cut up by veins of the granite, none pass into the Old Red strata. The author considered that the evidence in favour of the intrusive character of the granite was incontrovertible.

On the Upper Silurian Rocks of Lesmahagow, by Dr. Robert Slimon.—Dr. Slimon gave an interesting historical account of the mapping of the Upper Silurian rocks of Lesmahagow, and of the discovery and determination of their remarkable crustacean fauna.

On the Age, Fauna, and Mode of Occurrence of the Phosphorite Deposits of the South of France, by J. E. Taylor, F.G.S.—The author visited the phosphorite caverns within the last two months, and gave an account of what he saw.

On a Deep Boring for Coal at Scarle, Lincolnshire, by Prof. E. Hull, M.A., F.R.S.—The boring, after penetrating the Lower Lias, New Red, and Permian, entered the Carboniferous formation at the depth of 1,900 feet. The Carboniferous Rocks bored through were grey sandstones, with plants and shales with anthracosia, &c., 55 feet; calcareous shales and earthy limestone, 65 feet; fine breccia, 4 feet; chocolate-coloured clay, 6 feet. This succession was very puzzling. The beds above the breccia were pronounced by Prof. Ramsay and the author, without any consultation, to be Yoredale Rocks, but since the breccia has been reached, Prof. Hull inclines to regard it as belonging to the uppermost beds of the Coal Measures. As the boring is still going on, it is hoped that something more definite may be discovered.

A feeder of water was tapped in the Keuper Sandstone at the depth of 917 feet, and a still more powerful one in the Bunter Sandstone, at 1,250 feet, sent a jet of clear water 4 feet above the ground. The water must percolate from the outcrop of these beds ten or twelve miles to the west, being prevented from rising by the presence of the overlying impervious Lias Clay.

On Tertiary Basaltic Dykes in Scotland, by R. L. Jack, F.G.S., of the Geological Survey of Scotland.—Mr. Jack exhibited a map showing the courses of all the dykes of this age traceable for any distance which have hitherto been mapped by the Geological Survey, and described their peculiarities, referring specially to their avoidance of faults and other obvious lines of weakness. One dyke crosses Scotland from Helensburgh to Grangemouth, while two others maintain a parallel course from the heads of the River Irvine to the head of the Tweed, a distance of nearly forty miles. It was pointed out that a number of the larger dykes tend to converge towards the peninsula between Lochs Riddun and Striven, where, however, no evidence of volcanic activity, either in the shape of lava-flows or plugged-up vents, is known to exist.

On certain Pre-Carboniferous and Metamorphosed Trap-Dykes and Associated Rocks in North Mayo, by W. A. Traill, M.R.I.A., of the Geological Survey of Ireland.—In the district between Ballycastle and Belmullet the rocks belong either to the Carboniferous age or are older and metamorphosed. The author distinguishes at least two sets of dykes, both being basaltic. Those of the newer set run in straight lines, traverse both metamorphic and Carboniferous strata, and appear to fill vertical fissures or to come up along lines of fault. The older dykes

disturb only the metamorphic rocks, occur chiefly in sheets, and are often crumpled and contorted, while fragments of them occur in a conglomerate at the base of the Lower Carboniferous. This set must therefore be pre-Carboniferous, while the upper set is post-Carboniferous, and possibly Miocene.

SECTION D.—Biology.

Department of Anatomy and Physiology.

Address by John G. McKendrick, M.D., F.R.S.E., Vice-President.

The Future of Physiological Research.

Bearing in mind the fact that one of the objects of the British Association is to interest the public in the advancement of scientific truth, it has been the practice of the presidents of the various sections to make some remarks of a general character, or to give a *résumé* of the recent progress of science in their particular department. I shall follow so far the examples of my predecessors. I shall not attempt to enumerate, far less to describe, the contributions made to anatomical and physiological science during the past year, because that would entail a long and wearisome report regarding investigations with which most of us are already acquainted by the perusal of those excellent summaries that appear from time to time in our scientific and medical periodicals. With the view of limiting the scope of this address, I propose to offer a few observations bearing generally upon some of the scientific and social relations of anatomy and physiology, with the view of interesting the public in what we have been doing, and what we hope yet to do.

These sciences present different views of the same great system of truth. Each can be conceived as existing independently, while at the same time the one science is the complement of the other. Anatomy is the science of organic form, while physiology is that of organic function. The anatomist investigates structure, its form, general arrangements, and laws, and he may include in his survey the purposes or functions which the structure fulfils. Recently an opinion has been prevalent, and has cropped up in various quarters, that anatomy is but a preparatory science for physiology. This opinion has probably arisen in consequence of the rapid growth of physiological science during the last twenty or thirty years. But there can be no doubt that anatomy has a *rôle* of her own by no means inferior to that of physiology. She has to educe the formal laws which determine the structure of organised bodies and their parts, and thus she establishes the basis for scientific classification and arrangement. Anatomy is the beginning, of course, of all medical education, and the ground work on which the practical arts of medicine and surgery are reared; but in a broader sense, the science has to do with the structure of every animal, from the simplest to the most complex, and from the facts obtained in the investigation of the structure of any animal, we are able to recognise the relationships it has with other animals, or, in other words, its position in the zoological scale.

Dr. McKendrick then proceeded to speak of the methods of anatomy, histology, the methods of physiology, the vivisection question, the importance of teaching biology, the practical aspects of anatomy and physiology, the importance of investigations on the physiological action of active substances, the relation of physiology to medicine; after which, with reference to the relation of physiology to psychology, he remarked that as physiology is intimately connected with psychology, or the science of mind, and as this department of physiological work has lately been his chief study, he may be allowed to refer to it a little more in detail.

Psychology may be divided into two parts: first, all those phenomena which we may include under the term mind properly so-called, such as feeling, volition, and intellectual processes; and second, the phenomena which are associated with, and which indicate the alliance between, mind and matter. Every mental act may be regarded in the present state of knowledge as having a double aspect—on the one side it is known to our consciousness, and on the other side it is the result of a number of physical processes occurring in the brain.

The Methods of Psychology.

In the investigation of mental phenomenon, two modes of inquiry have been followed: first, that of introspection and reflection, in which the investigator looks within himself for the facts of his experience; and second, that of the examination of physiological processes which coincide with sensorial or mental changes. It is evident that the first of these methods, usually called the subjective, is open to the objection that by it a mind attempts to observe its own operations, and that the proceeding is somewhat analogous to asking a machine to investigate its own mechanism. This objection urged in other words by Comte, Maudsley, and others, may be answered by replying that the subjective method does not attempt to explain the physiological phenomena concomitant with mental states, but the laws which regulate these mental states themselves. Suppose a complicated machine possessed consciousness, I can readily understand that by the exercise of this consciousness it might be unable to discover the relation and mechanism of its own parts, because in attempting to do so the machinery would be so interfered with as to prevent normal action; but it might still be able to study the products of its operations. I do not, therefore, decry this old method of psychological research as it is so much the fashion to do in these days. Apart altogether from the philosophical speculations and systems of philosophy founded upon them, I think many data accumulated by such men as Locke, Berkeley, David Hume, Thomas Reid, Dugald Stewart, Thomas Brown, Sir William Hamilton, and James Mill, have as good a right to be considered correct as many of the quasi-metaphysical conceptions of physical science. Subjective inquiry carried on by such men cannot be given up as a mode of psychological research. It may not carry us much further than it has done, but it has rendered good service already, and may possibly do more.

But, on the other hand, the objective method appears to me to be the one which, in future, will be principally cultivated, and it is for this reason that, as a physiologist, I wish especially to refer to it.

It is the business of physiology to supply psychology with information regarding physical processes occurring in the nervous system; and it is one of the special features of the physiology of the present day to direct attention to the physical side of mental phenomena. No doubt Aristotle, Hobbes, and Hartley incorporated into their psychological theories much that was purely physiological; but in their days the physiology of the nervous system was in a crude state, and, consequently, did not lead to great results. In comparatively recent times, a new inductive and experimental department of science has arisen, the nature of which is indicated by the term physiological psychology, and which is being diligently cultivated by numerous workers, both at home and abroad. In our own country the writings and researches of Herbert Spencer, Alexander Bain, Dr. Laycock, George Henry Lewes, Dr. Maudsley, Dr. Carpenter, Alfred Barratt, and James Sully, and on the continent those of Fechner, Helmholtz, Wundt, Hermann Lotze, Taine, Donders, Plateau, and Dalboef, have excited much interest, and have led to the formation of a new school of thought.

I think it right to mention here specially the name of Prof. Laycock, who has done more, in my opinion, in this field of inquiry than any other member of the medical profession of this country in our time. His teaching has largely contributed to our present humane methods of treating the insane; he has attracted year by year some of the best students of the University of Edinburgh to this important department of medical practice; and his earlier writings incontestably show that, many years ago, and prior to most of the writings of those great men whose names I have just enumerated, he not only recognised the value of physiological research with regard to mental phenomena, but made important contributions himself.

Physiology has thus encroached on psychology, and is attempting to supply from the objective side an explanation of at least the simpler mental phenomena. As a proof of awakened interest in this department, one of the features of the past year has been the appearance of *Mind*, a quarterly journal of psychology, edited by my able friend Prof. Croom Robertson of University College. In the prospectus of this journal, it is stated that "psychology, while drawing its fundamental data from subjective consciousness, will be understood in the widest sense, as covering all related lines of objective inquiry. Due prominence will be given to the physiological investigation of nerve-structure." This quotation indicates the view which the editor takes of the relation of the two sciences, and already valuable papers have appeared on subjects connected with physiological psychology, from the pens of Sully, Lewes, Wundt, and others.

Now a certain class of thinkers are alarmed by work of this kind. They are afraid of the tendency "to represent the mental fact as a physical fact," and they are inclined to shut their eyes to the physical facts connected, undoubtedly, with psychological processes, and to be contented with the study of subjective

phenomena. But as most admit that there are two aspects in which mental phenomena may be viewed, why should not both be looked at carefully? If it be also admitted, that it is impossible to connect any physical process (supposing we knew it) occurring in brain cells with an act of consciousness, what is the use of taking a one-sided view of the phenomena in question? Why not study both sides of the problem, and give up the attempt at reconciliation, which is entirely beyond the pale of our faculties? This mystery of mind and matter has puzzled thoughtful men from the earliest times. Some have attempted a reconciliation. They have reasoned in a circle, so that most people, after perusing their works, are no nearer an ultimate solution than they were at the beginning. We always come back to this view of the case, namely, that every fact of mind has two aspects, a physiological and a psychological. That is one way of looking at the problem, and it is the one which, in the present state of knowledge, personally I prefer. But there is another. Thus, as has been well argued by Mr. George Henry Lewis in his recent work, "Problems of Life and Mind," two very different descriptions may be given of one and the same mental activity. The one may be expressed in the language of psychology, which is the language we commonly use to describe our feelings; the other may be stated in the language of physiology, a language intelligible only to those acquainted with the present state of physiological research. He says: "All that we have to guard against, is the tendency to mistake difference of aspect for difference of process, and to suppose that changes in feeling can exist independently of changes in the organism, or that any change in the organism can be effected otherwise than by some previous change." This way of stating the question may be more satisfactory to some minds. At all events, it is a fair attempt to solve the puzzle of our present state of existence, in which we are constantly brought face to face with the antithesis of object and subject.

Abandoning these speculations which are fruitless in practical effects, let me now endeavour very briefly to indicate the lines of inquiry in the domain of physiology, along which progress has been and may be made in the attempt to solve psychological phenomena; and I wish it to be understood that I do not take these in any logical order, but merely adduce them by way of illustration. It will also be my aim not so much to describe what has been done in the past, as to indicate what remains to be done in the future.

Research in Physiological Psychology.

First of all, then, it is quite evident that all researches on the general physiology of the great nerve centres are of paramount importance. Such researches as those of Hitzig, Fritsch, and Ferrier on the excitability of the cerebral hemispheres, supplying new ideas regarding the mechanism of the brain as a compound organ; of Wundt on central innervation and consciousness, in which he discusses in a manner never before attempted, the phenomena of reflex excitation; of William Stirling on the summation of excitations in reflex mechanisms; of various French physiologists on the mode of action of ganglia in insectæ; and of many others, are all recent important contributions to this department of science. Here, however, we have to confess that we have little accurate information regarding the minute structure of the parts involved, and consequently no anatomical basis on which to found our views. We have a general idea of strands of nerve-fibres and groups of nerve-cells of various forms, but we have no precise knowledge of the relative quantity of these, or of the relations of one group of nerve-cells to another group. We are unacquainted with any peculiarity in structure, for example, by which even an accomplished histologist could identify three microscopical sections as respectively portions of the brain of a man, of a monkey, and of a sheep. All this has still to be worked out. Every little area of brain-matter has to be surveyed and carefully described. Supposing this were done in the case of the human brain, and of the brains of the higher animals, the same must be attempted with the brains of animals lower in the scale. I can then conceive a grand collection of facts which may throw light on the intricate working of different kinds of brains, and, perhaps, afford a rational explanation of certain psychological characters.

Suggested Investigation.

What I mean may perhaps be better understood by a research, which I would suggest by way of experiment. No one who has kept an aviary of small birds—say a collection of our native and foreign finches—can have failed to observe marked differences of character and habits among different members of the same genus, and even among different members of the same species. One manifests cunning, another combativeness, a third kindness to smaller brethren, a fourth bullies all about him, a fifth may usually be quiet and peaceable, but occasionally gives way to uncontrollable rage, and so on. The question arises, then, Have these psychological peculiarities any organic basis, any explanation in the structure of the brain? or, are we to rest satisfied by asserting that these peculiarities are due to the action of some kind of psychical principle regarding which we know nothing? I have little doubt most will agree that these psychical characteristics of birds depend on peculiarities of brain structure the result of hereditary transmission through many generations. If so, here we have an opportunity of examining the microscopical structure of small brains, relatively simple, and easy of manipulation, with the view of ascertaining whether or not there are any structural differences which will account for these differences in psychical character. This is a line of inquiry likely, in my opinion, to establish an organic basis for a comparative psychology.

After referring to recent researches on the chemistry of the brain, Dr. McKendrick proceeded to refer to those on the physiology of the senses, which afford another series of data for the psychologist. These researches may be said to be of three kinds—(1) inquiries into the anatomical and physiological mechanism of the sense organ itself, such as, in the case of vision, the general structure of the eye as an optical instrument, and its movements by the action of muscles, so as to secure the conditions of monocular or binocular vision; (2) inquiries into the nature of the specific action of the external stimulus upon the terminal organ of sense, and the transmission of the effect to the brain; as, for example, the action of light on the retina, and transmission along the optic nerve; and (3) experiments in which various stimuli are permitted to act under certain conditions on the terminal apparatus, and the result is observed and recorded by the consciousness of the experimentalist himself, as in researches on colour, duration of impressions on the retina, positive and negative after-images, &c. By these three modes of inquiry a large number of facts relating chiefly to the senses of hearing and vision have been collected; and most of these facts, inasmuch as they assist him in understanding the conditions of sensory impressions and sensational effects, are of importance to the psychologist.

Measurement of Time in Sensory Impressions.

The next step of importance made by physiology into the domains of psychology is the measurement of time or duration in sensational effects.[1] This has been carefully measured by objective methods. Speaking generally, the time occupied from the commencement of the action of the stimulus to the termination of a sensation, may be divided into four portions, each of which has a certain psychological interest:—First, an interval of time is occupied by the primary physical change produced by the stimulus. During this interval, called the period of latent stimulation, no effect is observed. Thus, when a motor nerve distributed to a muscle is stimulated by a short electrical shock, about 1-60th of a second passes before the muscle contracts. Second, when the change in the nerve or terminal organ has begun, a second interval of time is occupied in the transmission of the impression to the nerve centre, which is succeeded by a third interval, during which changes occur in the nerve centre, and the result of which is a sensation. The time occupied in transmission, or the rate of conductivity in nerve, is tolerably well known, being at the rate of about 200 feet per second in the nerves of man; but the time occupied in the production of the sensation in the centre has not yet been clearly ascertained, owing to the difficulty of supposing such a sensory nerve centre to be, previous to the stimulus, in a state of absolute inaction. Lastly, it has been found that when a nervous action of any kind has been initiated by a stimulus, it goes on for some time after the stimulus has ceased to act. This prolongation of the sensation may be well studied in the case of impressions on the eye, where the time of the duration of the impression has been measured by Helmholtz, Plateau, and others. These distinguished observers also found that the length of time occupied by the after effect varied according to the intensity of the light. Thus, after a weak light, the unchanged impression lasts longer than with a strong light. A strong illumination is followed by an after impression fading sooner than with a feeble

[1] In the following observations I am much indebted to the essays of Mr. James Sully, contained in his volume, 'Sensation and Intuition." (London.)

stimulus; the result being that, so far as the retina is concerned, it comes to the same thing whether an intense light acts for a brief time, or a faint light for a longer time.

Exhaustion of Nerve or Sensory Organ.

This line of research has also made it possible to measure the time required for exhausting a nerve or sensory organ. When, for instance, a limited area of the retina has been stimulated for a certain time, and the stimulus has been removed, the after positive effect, due to increased excitation of the parts, disappears, and is followed by a negative effect, due to temporary diminution of the sensibility of the parts, in the form of what is called the negative after-image. Suppose, for example, an area of the retina be acted upon for a period of from five to ten seconds, and the stimulus be then removed, the so-called positive after-image vanishes quickly, and the negative after-image, frequently of a complementary colour to that of the exciting cause, appears, and lasts for a short time, gradually fading away as the nervous parts recover from the effects of the stimulus. Similar phenomena may be observed in studying the durations of sensations of tone, which I have frequently perceived in experiments made by myself; but it is more difficult to identify, by description and designation, the after effects in the case of audition than in the case of vision. Probably it may be found still more difficult to notice these after sensations in the other senses, although in all there is often the experience of a lingering feeling after the cause has been removed, which no doubt has its place in those transient sensations which assist in filling up the spaces, as it were, in our conscious life.

In experiments upon a sensory organ, such as the retina, a little consideration will show that it is almost impossible to ascertain the effect of a stimulus upon a retina which has never before been affected. This difficulty has been felt by all experimenters. Molecular action in such a structure has been in operation from the very beginning, and such action, if of sufficient intensity, must produce a certain effect on the conducting tract, and on the recipient centre. This effect, although of too weak intensity to produce those changes which result in consciousness, must be taken into account in the measurement of the intensity and duration of sensory impressions. Thus the eye has a light of its own due to changes in the retina, although this may never be conscious to us as a luminous impression. This conception of the state of matters in a terminal organ such as the retina, when applied to actions going on in the brain, at once indicates that similar actions, or rather that similar states of unrest, of change, variation, and modification, are going on in these deeper parts which may never result in consciousness, *per se*, but which altogether may have an influence on our mental existence comparable to that of the feeble impressions constantly transmitted to the cerebrum from the viscera, sometimes termed the internal senses.

Relation between Strength of Sensation and Magnitude of Stimulus.

Having shown that sensory impressions are distinctly related to time, the next advance made by physiologists was to prove that there was a relation between the strength of the sensation and the magnitude of the stimulus. Here there are difficulties in explaining what is meant, because language fails. We have no words to discriminate ideas which hitherto have related to two distinct fields of knowledge—the objective and the subjective. To speak of the strength or magnitude of a sensation seems to be using terms applicable only in another region, and quite inapplicable to psychological phenomena, although no one has any doubt in distinguishing the intensity or magnitude of one pain from that of another. There is no difficulty in understanding the phrase-magnitude of the stimulus. A weight of ten pounds is greater than that of one pound, light from ten candles of equal size is more than that given out by one, and the tones of a violin of equal pitch and quality, may vary in intensity according to the pressure of the bow on the string. It is difficult, however, to obtain an absolute measurement of variations in sensation, which is, of course, a subjective phenomenon. This can only be done by varying the objective cause, by observing a large number of instances, and by expressing variations in the subjective phenomenon in terms applied to variations in the objective cause. If the average result obtained from a large number of instances indicate any ratio between the magnitude of the stimulus and the subjective phenomenon, then we may conclude that there is a relation between the two.

This mode of inquiry, first originated by Prof. E. H. Weber in his celebrated experiments on tactile impressions (and which were first introduced to notice in this country by Prof. Allen Thomson), was afterwards carried out by his colleague Prof. Fechner, and has been subsequently elaborated by Prof. Wundt. It has led to various remarkable results, the chief of which are—(1) That in the case of each sense there is an upper and a lower limit, beyond which the amount of stimulus produces no appreciable difference of effect; and (2) that within this range there is a definite ratio between the stimulus and the amount of the sensation. The upper limit beyond which an increase of external stimulation is not followed by any observable increase in sensational effect, was first observed by Prof. Wundt. The lower limit has been noted by many observers, and it is indicated in almost every physiological text book. Now it does not matter much to us in taking a general view of things, what the limits are, provided we are sure that such limits exist, inasmuch as it indicates another element of proof that psychological phenomena, so far as sensation is concerned, occur within certain physical limits.

Fechner's Investigations.

The next step naturally was to establish the ratio between the magnitude of the stimulus and the magnitude of the sensation. To do this directly is impossible, as any estimation of the amount of sensational effect following a given stimulus would probably be erroneous, because our perceptions are usually qualitative and only rarely, and never absolutely, quantitative. Fechner recognised this fact, and he employed for the solution of the problem various methods by which he measured not sensations themselves, but the amount of discriminative sensibility between two sensations produced by *stimuli* of unequal magnitudes, and he studied the ratio between the difference of weight and the absolute quantity of the stimulation. By varying the amount of the stimulus in every possible way, he eliminated the chances of error, and arrived at definite results. These results he formulated into a general "psycho-physical law," which may be expressed in various ways. Mathematically it may be put, that "sensation increases in proportion to the logarithm of the stimulus." Now "logarithms increase in equal degrees when the numbers so increase that the increment has always the same ratio to the magnitude of the number." It may be put in another way by saying that "the more intense a sensation the greater must be the added or diminished force of stimulation in order that this sensation undergo an appreciable change of intensity." The mode of arriving at some of Fechner's results may be better understood by an experiment which any one can repeat. In the case of muscular sensation, suppose two weights A and B: we wish to ascertain the least difference between these perceptible by the muscular sense, say when we lift them in the hand. Let it be so arranged that both weights are composed of different pieces, so that the one may be made less or more than the other at pleasure. If A and B be nearly equal in absolute weight, the person on whom the experiment is made will judge them to be of equal weight. Let weights be now added to B until the difference between A and B becomes perceptible, and as a test, let the weights be again removed from B until, in sensational effect, A becomes again equal to B; let the same experiment be repeated with weights of different absolute amount, and it will be found that there is a distinct ratio between the absolute weight and the weight that had to be added to it or taken from it to produce the least perceptible difference of impression of whatever these weights may be, up to the limit, of course, which I have already noticed. It will always be found that the additional or subtracted weight is one-third that of the absolute weight—a fraction which indicates the degree of intensity of the stimulus required to produce the lest perceptible feeling of difference of sensation, and may be termed the *constant proportional* of that kind of sensation. This fraction, in the case of sensibility to temperature, Fechner found to be one-third; Renz, Wolf, and Volkmann arrived at the same fraction with regard to auditory impressions; and various observers have found that in visual impressions it is one-hundreth.

Now the intensity of sensation depends on two conditions: (1) the intensity of the excitation; and (2) the degree of excitability of the sensory organ at the moment of excitation. But suppose the excitability of the organ equal on two occasions, the intensity of the sensation does not increase proportionately to the increase of the excitation. That is to say, suppose we bring into a dark chamber a luminous body such as a candle—it produces a certain luminous sensation; then introduce a second, third, and fourth—the excitation is double, triple, or quadruple; but experiment shows that the increase in the amount of the sensation is much less; in other words, let the stimulus increase from 10 to 100 times, and from 100 to 1,000 times, the sensation will be

only one, two, and three times stronger. The importance of the discovery of this remarkable law is, that it shows a distinct mathematical relationship between stimulation and sensation. Possibly it may be found to have applications to other psychological phenomena. May it not vary in different animals, and even in different individuals?

Criticism of Fechner's Method.

It is quite noticeable, however, that in the case of each sense, the law did not hold good throughout the whole range of variations in intensity of stimulus; and it is not surprising, when we consider the complexity of the conditions, that such should be the case. All of these experiments were made in the case of visual impressions, for example, on the living eye, connected by the optic nerve with the brain; and it is manifestly impossible, as has been remarked by Hermann, "to localise this relationship between sensational effect and variation in amount of stimulus, which has been called the psycho-physical law of Fechner." Between the sensational effect and the first contact of the stimulus, there are a series of complicated processes occurring in retina, nerve, and brain, processes undergoing incessant modification by the interchanges between these tissues and the warm circulating blood. In which of these does this relation between stimulus and conscious state occur—in retina, in optic nerve, or in brain? The only method of answering this question, so far as I know, is to examine the effects of stimulation upon these parts separately. It is manifestly next to impossible to do this in the case of the optic nerve and the brain; but by the method pursued by Holmgren, in Sweden, and by Prof. Dewar and myself in this country, it can be done, so far as the retina is concerned. In carrying out this method, Prof. Dewar and I found that light produced a change in the electrical condition of the retina in an eye removed from the head or kept in normal conditions, and we ascertained that the general phenomena of this change corresponded with our sensational experiences of luminous impressions. We were, therefore, entitled to assume that the change in the electrical conditions of the retina, produced by the action of light, might be regarded as a phenomenon intimately related to those changes in the brain which result in consciousness of a luminous impression. Consequently we had an opportunity of ascertaining whether or not Fechner's law agreed with the effects of a stimulus of light in altering the electrical condition of the retina, and we found that it did so. The inference, therefore, is that the relation between degree or variation in stimulus and the corresponding sensation of a luminous impression, is a function of the sense organ or retina.

Mode of Investigating the Sensory Organ Itself.

I may here remark that this mode of inquiring into sensory impressions has by no means been exhausted. The subjective method of observing sensational effect under the stimulus of light from revolving discs, by the contrasting of colours, by comparison of auditory sensations produced by tones of different intensity, pitch, and quality, is always open to the charge that the results may not be due to specific histological structure of the sense organ, as is almost invariably assumed, but to structure of the recipient of impressions from the sense organ, namely, the brain. The only way of proving that the effects are due to structural peculiarities of the sense organ is to examine the effects of stimuli applied to the sense organ separated from the brain by some method the same or analogous to ours. If in these circumstances the sense organ give results similar to those observed in the phenomena of consciousness, then we may assume that these results are due to specific peculiarities of the sense organ, and not to the brain. If, on the other hand, the results do not agree, then we must look in the brain for the mechanism by which these different results are produced. Thus I have always held, that as there is little or no histological evidence of complexity of structure in the retina capable of accounting for the theory of Thomas Young regarding the perception of colours, or of the facts of colour-blindness, or of the sensibility of different zones of the retina to lights of different colours, we may have to look to the complex structure of the corpora quadrigemina, cerebellum, or some portion of the cerebral hemispheres for an explanation of these facts. It may be objected that such scepticism simply removes the difficulty a little further back, but I think it is better to search for facts than to be contented with an hypothesis.

Conclusion.

Time will not permit me to discuss other researches in this field of inquiry, nor the interesting speculations which have sprung from them, but I think I have said enough to show the line of advance in this direction.

True it is that apparently the physiological causation of many mental phenomena may be, in its precise nature, inaccessible to direct proof, but it is our duty as physiologists to push legitimate research as far as it will go. I would remark also that such researches are not incompatible with those spiritual ideas, matters of faith and not of science, which are the basis of our most cherished hopes. They demand, however, caution in the scrutiny of facts, and judgment in drawing conclusions from them. More than in any other kind of scientific labour, perhaps, it is of the utmost importance here to keep the mind unbiassed, a task by no means easy. To maintain a calm unprejudiced attitude to inquiries which seem to demand a change of opinion regarding what was supposed to be final, requires an effort which varies in different persons. Some find it comparatively easy to do so, while others succeed only after a severe struggle. Still it is the state of mind which a man true to science ought to aspire to, so that while he will not be blown about by every wind of doctrine, he may be ready to accept what is apparently true when he has had it clearly put before him.

In conclusion, let me observe that it would save not a little heart-burning, and might possibly remove acrimony from various scientific and social controversies, could we only remember that it is not very probable that we, in this nineteenth century, have arrived at the final solution of many problems which have puzzled wise men from the earliest times. Probably we have got nearer the truth, but it is presumptuous to suppose that we have reached the ultimate truth. Many hypotheses much in favour at present may turn out to be inadequate. Still if they serve as stepping stones to something better, and to more rational conceptions of the mysterious phenomena about us, they will have done good service. In the meantime it is our duty vigorously to prosecute research, in all departments, pushing ahead fearlessly, and with that enthusiasm which is the prime mover in all great deeds, so that we may be able to transmit our department of knowledge to posterity not only less burdened with error, but with many additions of truth.

Prof. Turner, of Edinburgh, gave an account of his researches into the structure of the placenta in mammals, and showed how forms originally supposed to be distinct and unconnected by gradations, were really but modifications of one fundamental type. Thus the obstacle to the reception of the theory of evolution, which had been supposed to be constituted by the various placental structures, did not exist. But it was difficult to see in many respects what causes had determined the evolution. In some cases it appeared that the great dilatation of blood capillaries in the uterus might be of advantage, because less force would be required for the propulsion of the blood. Again, in the upward ascent, there was complication of the placental structures with restriction of area; and he supposed that with this restriction there would be a diminished danger of hæmorrhage after parturition, and consequently greater safety.

Mr. F. M. Balfour, Fellow of Trinity College, Cambridge, read a paper *On the Development of the Protovertebræ and Muscle-plates in Elasmobranch Fishes.* The most important points on which he laid stress were the origin of the notochord from the hypoblast, the splitting of the mesoblast from the hypoblast as two distinct lateral halves, the consequent appearance of the body cavity at first as two cavities, the extension of the body cavity on each side up to the summit of the muscle-plates, and the derivation of a large portion of the voluntary muscular system from the splanchnic or visceral layer of the mesoblast. He compared these embryological facts with many occurring in the Invertebrates, especially in Sagitta, in Brachiopods, and in Echinoderms, showing how it was possible to unify them by adopting Haeckel's gastræa theory, and by no other method. Dr. Allen Thomson warmly commended Mr. Balfour's researches, saying that it was quite a new thing for such a continuous series of embryological papers of great importance to proceed from a British investigator.

Mr. G. J. Romanes, M.A., gave an account of his further researches on the physiological functions of the Medusæ this summer. To this we shall return.

Prof. Haeckel described two of the simplest forms of animals with two layers in their body-wall—Haliphysema and Gastrophysema. They were Cœlelenterata of the simplest type; the first form had one body cavity; in the second it was partly divided into two cavities, whereof one was specially appropriated to the formation of ova, the other to nutrition. If there had

been pores in the body-wall he should have referred both forms to sponges. Their development showed that they arose strictly in conformity with the Gastræa type. He then gave some account of the mode of development of the chief animal stocks, as explained in the "History of Creation." Dr. Allen Thomson said that Prof. Haeckel had been regarded in many quarters with somewhat of the same suspicion that had greeted the first promulgation of Mr. Darwin's theories, and he was considered one of the most rash and daring speculators of the day. Those who had listened to his exposition would probably take a different view, and see how much of sound observation went to the establishment of his theories. In so extensive a field as that over which Prof. Haeckel's views carried him, he might be sometimes led into error, and might possibly be widely wrong, but at the same time they could not but admire the manner in which observation of fact was always placed as the basis of his theory.

Dr. D. J. Cunningham read a paper *On the Spinal Nervous System of the Cetacea.* He found that while great similarity prevailed between their cervical and dorsal nerves and those of other mammalia, the nerves of the lumbar and caudal regions differed widely. The superior and inferior divisions of those nerves in cetacea were of nearly equal size. Two great longitudinal cords or trunks are formed by their union on each side of the vertebral column, and these become situated on either side of the spines of the vertebræ, and on either side of the bodies below the transverse processes. These great cords supply the four great muscular masses which act upon the tail.

Prof. Burdon-Sanderson gave an account of his further researches *On the Electrical Phenomena exhibited by Dionæa muscipula (the Fly-trap).* He had accurately investigated the phenomena by means of the electrometer. He found that normally the whole leaf with the petiole was somewhat negative, but that when excited by a stimulus, an electrical change took place throughout, making every part more negative; the greatest change was on the external surface of the leaf immediately opposite to the three sensitive hairs. There was no relation between the pre-existing currents and the electrical disturbance consequent on stimulation. The period of latent stimulation was about one-sixth of a second; the period during which the disturbance lasted was one second, more or less. As the leaf becomes fatigued, the period of latency gradually increases to one second and three-quarters, and then most likely the next stimulation would produce no effect. The change appears to be a function of the protoplasm of the parenchyma of the region out of which the sensitive hairs arise. Certain of the characters of the change are similar to those presented by muscle and nerve. Why the variation should be a negative one, Prof. Sanderson had no idea.

Prof. Struthers described the finger-muscles of several whales. He concluded that such muscles existed in the whalebone whales, but in ordinary teethed whales they were merely represented by fibrous tissue. These muscles existing in the true bottle-nosed whale had a special interest, as the teeth in that whale were rudimentary and functionless. He had found these muscles in the forearms of whales largely mixed with fibrous tissue, so the transition was easy. He also gave an account of dissections of the rudimentary hind-limb of the Greenland right-whale. Prof. Macalister, of Dublin, expressed his opinion that the whales were not of very ancient origin, for he thought the existence of the rudimentary limbs tended to show that a sufficient length of time had not elapsed since the use of the limb was essential to the earlier animal, to produce its complete obliteration.

Mr. C. T. Kingzett read a paper *On the Action of Alcohol on the Brain.* He said the question of what became of alcohol taken into the system had been extensively studied. Thudichum was the first to determine quantitatively the amount of alcohol eliminated by the kidneys from a given quantity administered, and the result he obtained was sufficient to disprove the elimination theory then widely prevailing. Dupre and many others continued these researches from which, according to Dupre, they might fairly draw three conclusions: (1) that the amount of alcohol eliminated per day did not increase with the continuance of the alcoholic diet, therefore all the alcohol consumed daily must of necessity be disposed of daily, and as it was certainly not eliminated within that time it must be destroyed in the system; (2) that the elimination of alcohol following the taking of a dose was completed twenty-four hours after the dose was taken; and (3) that the amount eliminated in both breath and urine was a minute fraction only of the amount of alcohol taken. In 1839 Dr. Percy published a research on the presence of alcohol in the ventricles of the brain, and, indeed, he concluded "that a kind of affinity existed between the alcohol and the cerebral matter." He further stated that he was able to procure a much larger proportion of alcohol from the brain than from a greater quantity of blood than could possibly be present within the cranium of the animal upon which he operated. Dr. Marcet, in a paper read before the British Association in 1859, detailed physiological experiments which he considered to substantiate the conclusions of Dr. Percy, inasmuch as they demonstrated that the alcohol acted by means of absorption on the nervous centres. Lallemand, Perrin, and Duroy had, moreover, succeeded previously in extracting alcohol from brain-matter in cases of alcoholic poisoning. But all these researches left them entirely in the dark as regarded the true action, if any, of alcohol on cerebral matter, and no method of investigation was possible until the chemical constitution of the brain was known. Thudichum's researches in this direction, together with some more recent and published investigations by Thudichum and the author, had placed within reach new methods of inquiry regarding the action of alcohol on the brain. In his research he (Mr. Kingzett) had attempted this inquiry by maintaining the brains of oxen at the temperature of the blood, in water, or in water containing known amounts of alcohol. The extracts thus obtained had been studied in various ways, and submitted to quantitative analysis, while the influences exerted by the various fluids on the brain had been also studied. These influences extended in certain cases to hardening and to an alteration in the specific gravity of the brain-matter. Water itself had a strong action on brain matter (after death) for it was capable of dissolving certain principles from the brain. It was notable that water, however, dissolved no kephaline from the brain. Alcohol seemed to have no more chemical effect on the brain than water itself, so long as its proportion to the total volume of fluid did not exceed a given extent. The limit would appear to exist somewhere near a fluid containing 35 per cent. of alcohol. But if the percentage of alcohol exceeded this amount, then not only a larger quantity of matter was dissolved from the brain, but that matter included kephaline. Such alcoholic solutions also decreased to about the same extent as water the specific gravity of brain substance, but not from the same cause; that was to say, not merely by the loss of substance and swelling, but by the fixation of water. Many difficulties surrounded the attempt to follow these ideas into life, and to comprehend in what way these modes of action of water and alcohol on the brain might be influenced by the other matters present in blood. On the other hand, it was difficult to see how any of the matters known to exist in the blood could prevent alcohol, if present in sufficient amount, from either hardening the brain (as it did after death) or dissolving traces of its peculiar principles to be carried away in the circulation; that was to say, should physiological research confirm the stated fact that the brain in life absorbed alcohol and retained it, it would almost follow of necessity that the alcohol would act as he had indicated and produce disease, perhaps *delirium tremens.* Dr. McKendrick said Mr. Kingzett's researches into the chemistry of the brain and the action of various agents upon it were a valuable step in the right direction. This was essential if the mode of working of the brain were ever to be understood; but it would be a long way from the knowledge of the dead tissue to the comprehension of its vital action. No doubt alcohol had a marked effect upon the convective-tissue elements in the brain. He suggested as a useful method of research the submitting a certain class of animals for a length of time to the action of a definite amount of alcohol, and then examining their brains to discover what effect was produced. The investigation was of very great importance as regarded the treatment of drunkards; no doubt in many cases where it was thought that they had to do with merely moral evil, there was a fundamental change in physical organisation. Prof. Burdon-Sanderson said the question was one that ought certainly to be taken up by Government, and the best men in the country should be engaged upon the inquiry. It had a most important bearing upon the welfare of the community and the diminution of human suffering.

Surgeon-Major Johnston, in a paper *On the Diet of the Natives of India*, came to the conclusion that the natives require much more nitrogen and carbon than Europeans, and also took much more salt, owing to the comparative absence of salt from the substances which form a large part of their food. The natives took more dry food than the Europeans, and those who lived on food from the tables of the Europeans enjoyed a considerably greater immunity from cholera than others.

Mr. Wanklyn read a paper *On the Effects of the Mineral Sub-*

stances in Drinking-Water on the Health of the Community. One of the questions which has often been asked is, Whether is it better to drink hard water or soft water? The reply which has been given is that at present we cannot tell, but that apparently the system can accommodate itself to either, and that a soft-water drinker is sometimes disordered when he begins to drink hard water. He wished to call attention to the opportunity that physicians had at present of discovering the effects of hard waters by reason of the great use that was being made of a very hard water, the Tannus water. Ordinary hard water might contain from 13 to 20 grains of carbonate of lime per gallon; but the Tannus water contained, roughly speaking, 100 grains of carbonate of lime and 200 grains of common salt per gallon, besides considerable quantities of carbonate of magnesia, chloride of potassium, and sulphate of soda. In the course of the discussion which followed the reading of Mr. Wanklyn's paper, Dr. Carr stated that in Kent, where the water was hard, he believed the amount of salts of lime was exceedingly beneficial to children, and the Kentish children were singularly well supplied with straight legs and good bones. Mr. Wanklyn stated that Kent water was one of the purest he had ever seen; average drinking-water contained ten times as much organic matter as the Kent water. The real objection to the latter was that it contained a large proportion of sulphate of lime; whenever it was met with in any volume it had something of the odour of rotten eggs, due to the presence of sulphuretted hydrogen and sulphate of lime. He questioned whether hard water, however useful for children, was altogether desirable at a later peried of life.

Dr. Paton's able paper *On the Action and Sounds of the Heart* gave an account of excellent experimental researches, by which he claimed to have proved that the ventricle in coming to complete contraction itself exerts a strain on the base of the distended aorta that produces the simultaneous reaction of the aorta, closing the valves and completing the wave. This is contrary to the usual view which considers that the aorta reacts after the conclusion of the ventricular contraction. The influence of this new conception on the comprehension of the sounds of the heart is important; for if a sound be produced in closing the semilunar valves, it must terminate the first sound of the heart, and cannot be the second sound. The latter arises after the first pulse-wave has terminated, and is synchronous with the diastole of the ventricle. In a series of experiments on the action of the denuded heart of the terrapene during the highest temperature of the season, when the action of the heart was strong and vigorous, Dr. Paton distinctly identified the first sound with the contraction of the ventricle and the reaction of the aorta, the sound being produced by the rushing of the blood through the orifice and terminated by the recoil of the aorta. The second sound, short, sharp, and acute, was produced by the contraction of the auricles sending the blood through the auriculo-ventricular orifices. The effect of these facts upon pathological sounds was followed out.

Among other contributions to this department may be mentioned Prof. Dewar's continuation of his important researches *On the Physiological Action of Light*, Dr. Urban Pritchard's paper *On the Termination of the Nerves in the Vestibule and Semicircular Canals of the Ear of Mammals*, and the same author's *Demonstration of a New Microscope adapted for showing the Circulation in Man.*

The five days' session of this department was fruitful in important memoirs on physiology, anatomy, embryology, and histology, showing that a considerable amount of good work is going on in this country. The discussions were of more than usual value, as many eminent anatomists and physiologists were present and took part in them.

Department of Zoology and Botany.

Among the botanical contributions was an interesting one by Dr. I. B. Balfour, entitled *Notes on Mascarene Species of Pandanus.* He said that no portion of the flora of the Mascarene Islands was more peculiar than the various species of the genus Pandanus, or screw Pines. There were many species endemic to the islands, but many species were found all over India, and they also extended into China and other places in the Malay Archipelago, and a few species were to be found in Australia. Of the twenty-two species which occurred in the Mascarene Islands, twenty were endemic to the islands; their generic characters were exceedingly well marked, and the definition of species was a very difficult matter. An investigation of the whole genus was very much wanted, but this had hitherto been rendered difficult by the want of knowledge of the Mascarene species. The descriptions of the first author who wrote anything about these Mascarene species were exceedingly short, and just now the confusion in regard to the whole genus was something extraordinary. There were nine species at least endemic to the Mauritius, and in the Bourbon they had record of four distinct species, three of which were peculiar to the island. He had examined the fruits and leaves of these plants, but the leaves afforded very few characters. They were diœcious plants, and the male flowers would furnish them with very good characters for distinction. Three species had been grouped together by their carpels never or at least very rarely being united. Two of these were endemic to Mauritius, and one to Bourbon.

Prof. W. C. Williamson gave an address on his recent researches on the structure of the coal plants, especially Calamites, Lepidodendron, and Sigillaria. He considered that the accurate determination of the true nature of each of the coal plants was of the utmost importance to the theory of evolution. He combated the view which would divide the genus Calamites into two, Calamites and Calamodendron. He described some new forms of lepidostrobi or cone-fruits of fossil lycopods, and concluded by showing the remarkable tendency of many of these coal plants to develop into a very uniform type, making it almost impossible to identify small fragments either of their wood or of their bark. Hence it was absurd to attempt to establish genera and species upon such unrecognisable fragments.

Mr. C. W. Peach read a paper *On Circinate Vernation of Sphenopteris affinis, and on the Discovery of Staphylopteris in British Rocks.* Mr. Peach has found *Sphenopteris affinis* in the black shale at West Calder, near Edinburgh, in a series of specimens showing its vernation from the earliest stage till the complete development of the plant; he believed that other observers had described several species of Sphenopteris from this one form in its various stages. The genus Staphylopteris, which he had also found at West Calder, was well known as occurring in the carboniferous rocks of Illinois and Arkansas. Prof. McNab gave an account of the structure of the leaves in several species of Abies (larches), which will be fully illustrated in the *Proceedings* of the Royal Irish Academy.

Prof. Leith Adams described the fossil remains of the Maltese caves, with especial reference to the gigantic land-tortoises, similar to those of the Galapagos and Mascarene Islands, but much larger still. Nevertheless they were very much alike in osteology, so that there had been great difficulty in determining that the species were distinct. Another notable animal was a dormouse as large as a guinea-pig, so numerous that five or six specimens could be obtained out of one spadeful of mould. Among the fossil-birds was a swan one-third larger than any modern one. Altogether 150 terrestrial vertebrates had been found in Malta, and it was impossible that they could have lived in that locality unless Malta was part of a continent.

Mr. Spence Bate, in continuing his report on the structure of the Crustacea, dealt especially with the eyes, pointing out that these organs were in some cases covered by, and received support from, the carapace, and in others they were supported by a jointed peduncle. The chief modifications of the appendages of the head were examined, and they led Mr. Spence Bate to the conclusion that the seven sections of which the head was composed should be regarded as completely different from the other parts of the body.

Dr. W. B. Carpenter reported the result of further researches *On the Nervous System of Antedon (Comatula) rosaceus*, and also read a paper by his son, Mr. P. H. Carpenter, *On the Anatomy of the Arms of Crinoids.* He maintained that the tract of tissue in the axis of the arms, by which motor impulses were conveyed to the arm-muscles, was equivalent to a nerve, although it did not present the microscopic structure of nerve-fibres.

Dr. D. J. Cunningham read a paper *On a Specimen of Delphinus albirostris* which he had procured this spring. Prof. Cohn, of Breslau, made a number of beautiful experiments to show the artificial formation of silica shells.

Prof. Young gave a description of the novel arrangements adopted by him in the new Hunterian Museum. The cases were arranged so that visitors could walk around them on the outside while curators or students were at work upon them on the inside. The cases were to contain skins, skeletons, soft parts, and fossil remains in close proximity, so that the whole of what was known about one series of forms might be brought together, instead of being scattered as usual. The fittings had been made with great skill by Messrs. D. and T. Robertson of Glasgow.

A discussion on spontaneous generation arose on a paper by Dr. Carmichael, of Glasgow, entitled *Spontaneous Evolution*

and the Germ Theory. Dr. Carmichael had made a considerable series of experiments, the results of which were generally confirmatory of those of Dallinger and Drysdale, and of Tyndall. Prof. G. S. Boulger read a paper *On Sex in Plants*, giving a comprehensive view of recently-acquired knowledge on the subject.

This department certainly did not produce papers ranging over more than a small portion of the field allotted to it. Some contributions of high merit were made, but in many departments of natural history no sign was made that any work was going on in the British Isles.

Department of Anthropology.

Mr. James Shaw read a paper *On Righthandedness*, expressing the opinion that there was a constitutional reason for the greater use of the right hand. Lefthandedness seemed very mysterious physiologically; it must be far more common than transposition of the viscera which had been supposed to account for it. In several cases of transposition of the viscera, the persons affected had been found to be right-handed. Another paper by Mr. Shaw was *On the Mental Progress of Animals during the Human Period*. In the discussion which followed Dr. Grierson mentioned an instance of intelligence which had come under his own notice. Five years ago a barrel was put up in his garden at the top of a high pole. The barrel was perforated with holes and divided in the centre. In the course of two days two starlings visited the barrel, and returned on the following day, and in about a week afterwards two pairs of starlings came and occupied it, and brought up their young. They were very wild starlings, and readily took flight when any person went near the barrel. In the second year four pairs of starlings occupied the barrel, and they were much tamer than the previous ones, and this last year there were a number of pairs of starlings so tame that they would almost allow him to take hold of them. They had now changed their mode of speaking, for the starlings in his garden frequently articulated words.

Mr. Hyde Clarke read a paper *On the Prehistoric Names of Men, Monkeys, and Lizards*, tending to prove that in early times and by some savage races at the present day, every word which was used as distinctive of man was likewise applied to other animals, but only to those which used their fore feet as hands, or in a distinctive manner. A paper contributed by Herr von Humboldt von der Horck was read by Mr. Hyde Clarke. The author was in charge of an expedition to the polar seas, and sent an account of the Laplanders and people of the north of Europe. He divided the Lapps into the nomadic or mountaineers, and the sea or fish Lapps. The nomads were stronger, healthier, and better developed, and rarely intermarried with the Finns or the Norwegian settlers.

Mr. Hyde Clarke's researches *On the Relations between the Hittite, Canaanite, and Etruscan Peoples and the Early Peruvians and Mexicans* were laid before the department. He believes that they really belong to one family, representing an early culture which became arrested. They had little community with the Semitic or Aryan types. Mr. J. Park Harrison dealt with the origin and meaning of the "Picture Writing" of Easter Island. He said that many of the tablets were gradually getting destroyed, and he called attention to the desirability of acquiring as many of them as possible, and of instituting a careful ethnographical exploration of Easter Island.

Mr. Bertram F. Hartshorne, late of H.M. Ceylon Civil Service, read a paper entitled *The Rodiyas of Ceylon*. The people treated of in his paper were a numerically small race, living in various isolated communities in the hill country of Ceylon. Their caste is the very lowest, and they have from time immemorial been regarded by the Singhalese people with disgust and abhorrence, their very name implying the notion of filth. The popular belief has commonly considered them to be either in some way connected with the Weddas, an aboriginal race of the highest caste, or else to be outcast Singhalese or ostracised Kandyans. There appears, however, to be no real ground whatever for either of these theories—the features of the Rodiyas, as well as their general *physique* and their craniology, marking them out as a separate and distinct race, no less than their customs and language. Their customs are distinguished by peculiar funeral ceremonies, and by sacrifices offered to two sorts of devils in cases of serious sickness; and their language, which is now in one of the last stages of decay, is of unknown origin and development, and can neither be classified as Aryan nor Dravidian. In all probability it represents the remnants of a more complete and extremely ancient language, although it possesses no separate alphabet, nor any literature. The earliest historical mention of the Rodiyas apparently occurs in the year 437 B.C., and they are expressly referred to by name in the year 204 B.C., and again in the year 589 A.D. in the ancient Singhalese chronicles. The condition of the people, however, has at all times been degraded, notwithstanding the fact that the males are invariably possessed of a fine *physique*, and the females are considered to be handsome. The peculiar social disabilities which have been imposed upon the Rodiyas by the uses of ages are now rapidly disappearing with the advance of civilisation, whilst at the same time the idiosyncrasy of the people themselves as well as their customs and their language, is gradually becoming merged in the more modern type of their Singhalese surroundings. The president (Mr. Wallace), in moving a vote of thanks to Mr. Hartshorne, said the Rodiyas were a race of people who, though in a degraded condition, yet possessed physical characters which seemed to show they were intellectually superior to the races who treated them in this manner. This might be another of those examples to which he alluded in his address, of a remnant very fast dying out—a remnant of one of those early higher races which had been overrun and overcome by a lower race intellectually, but more energetic, and had been reduced to an extremely degraded position. It was also a valuable example proving that degradation long continued did not alter to any great extent the physical features of the race. Though they had been for ages in this degraded condition they retained a fine type of face, almost equal to many European forms.

Mr. William Harper contributed a paper *On the Natives of British Guiana*, who were generally said to belong to five tribes, namely, the Arawacks, the Caribs, the Accawoi, the Macuri, and the Warans. Representatives of several other tribes were, however, frequently met with on British soil. These people were merely remnants of a few barbarous tribes found, for the most part, between the Amazon and the Orinoco. It was extremely difficult to obtain any information as to the origin of these tribes; and the general result of the author's investigations was that, though it did not now admit of proof, it was very probable that all the Brasilio-Guarani tribes came from the north, though not at the same time. Of the tribes in British Guiana, the Warans and Macuri had probably been longer in the country than the Caribs, Accawoi, and Arawacks. These tribes differed a good deal from one another in their language, characteristics, and habits, but not in their outward appearance or mode of living. The author suggested that light might be thrown on the origin of these tribes by collecting fac-similes of the rock-writing to be found among them, and comparing them with similar writing to be found in other parts of America, especially in the valley of the Mississippi.

Mr. Kerry Nichols read a paper *On the New Hebrides, Banks, and Santa Cruz Islands*. The natives inhabiting these islands seemed to owe their origin to the same stock from which the western and southern portion of New Guinea and the islands lying immediately to the southward of that country appear to have been peopled. The stock was evidently Papuan, and had, by its numerous and wide-spreading branches, not only extended itself over the islands of the coral sea, but as far east as the Fijis, in which latter country, however, the race had evidently received a great infusion of Malay blood. Whatever opinion might be formed on the identity of the present race, the striking resemblance in person, feature, language, and customs which prevailed throughout, justified the conclusion that the original population issued from the same source, and that the peculiarities and characteristics which distinguish the tribes or communities on different islands had been mainly brought about by long separation, local circumstances, and the intercourse of foreign traders and settlers. Physically considered, these people were a well-built, athletic race of savages, who appeared to inherit, in a very marked degree, all the characteristics of the Papuan race. The men average about 5 feet 6 inches in height, are erect in figure, with broad chests and massive limbs, which in many instances display great muscular development. The colour of the skin was usually of a dark reddish brown, but sometimes it was quite black, and was often covered with a short, curly hair, especially about the breast, back, and shoulders. He saw several instances in the Island of Tanna where the body was almost completely covered in this way. They had well-formed heads, the cranium in the majority of instances betokening a fair degree of mental development. The hair, which formed one of the most remarkable features of this race, was distributed thickly over the head in the form of small spiral curls, and when allowed to grow in its natural way

had a woolly appearance, and resembled at first glance that of the African negro, but it was in reality much finer and softer. The beard was worn short, and usually trimmed, with a tuft beneath the chin. They shave with the teeth of the shark, an oyster shell, or a piece of bottle glass, and perform the operation with the skill of accomplished barbers. In the northern islands the men went completely naked; but in the southern islands, where the climate was slightly cooler, they affected a scant covering, after the fashion of the primitive fig-leaf. They were fond of decorating the head with flowers and feathers, and of tattooing the face with red and blue pigments, which imparted to them a savage and ferocious look. All things considered, the physical condition of the islanders did not appear to manifest any sign of degeneration. A very complete account of the social and intellectual condition of these islanders was given. The slight idea of religion possessed by the islanders might be described as the most primitive form of Paganism. On some of the islands they worshipped rude idols of wood, while in others they seemed to put implicit faith in imaginary gods who were supposed to inhabit the highest mountain tops. The dread of evil spirits and demons was universal among them. The natives of each island had a distinctive dialect of their own, and even the various tribes inhabiting each island had also distinct and separate dialects.

Mr. W. Pengelly, F.R.S., gave an account of the contents of an urn which had been found in a field near Chudleigh in Devonshire. The urn contained a large number of pieces of pottery supposed to be Roman, and a number of calcined bones which were the bones of goats or sheep. This was the only occasion, as far as he knew, in which the bones of animals had been found in such urns.

Dr. Knox read a paper *On Bosjes Skulls*. One of his specimens had a capacity of only sixty-four cubic inches; the longest measured seventy-four cubic inches. The skulls belonged to the long-headed type, though not of the longest. The skeleton to which one of the skulls belonged, was remarkable for the wedge-like shape of the pelvic bone, which was also very deep.

Dr. Allen Thomson exhibited and described two skulls from the Andaman Isles; and referred to the custom the natives had of preserving portions of their friends' skeletons and wearing them as ornaments. The skulls of their husbands were actually worn upon the shoulders of widows.—Prof. Cleland described the skull of a Sooloo Islander.—Dr. McCann, in a paper *On the Origin of Instinct*, brought forward well-known objections to Mr. Darwin's explanations, referring to the descent of bees, the first birds hatching eggs, &c.

Nearly the whole of one day was occupied by the reading of a paper by Prof. Barrett, of Dublin, *On some Phenomena Associated with Abnormal Conditions of Mind*, on which an excited discussion arose. Many phenomena of mesmerism, clairvoyance, and spiritualism were alleged, and Mr. Crookes, Mr. Wallace, Lord Rayleigh, and Dr. Carpenter expressed opinions which are well known, based on facts witnessed by themselves.

The work done in this department does not compare well with the result at Bristol last year. Scarcely anything of importance was brought forward in prehistoric anthropology. Some good accounts of savage tribes of the present day were given; but otherwise the scientific value of the department is this year comparatively small. The concluding portion of Mr. Wallace's presidential address is perhaps the most noteworthy feature in anthropology, as exhibited at Glasgow.

SECTION E.—Geography.

There were an unusual number of papers of general interest and importance in this as well as in Sections F. and G., and we therefore regret that our space does not permit of reporting them at length.

Mr. Octavius Stone read a paper *On his Recent Journeys in New Guinea*. The island, he said, extended in a south-easterly direction for a distance of over 1,400 miles, having a maximum width of 450 miles and a minimum of only 20. The neighbourhood of the Baxter River and the entire shores to the west of the Papuan Gulf for an average of 100 miles inland were low and more or less swampy, being intersected by water-courses and covered with forests of mangrove trees. This part of the country was thinly populated by the Dandé Papuans, who in consequence were subjected to periodical raids from the adjoining islands of Borgu, Saibai, and Daun, the invaders generally returning victorious with the heads or jawbones of their slaughtered victims. The only trace of cultivation he saw was 80 miles up the river, where a space of six acres had been neatly fenced round, and planted with yams, taros, sugar-cane, and tobacco. Outside the inclosure were two or three uninhabited bark huts, which appeared to afford shelter to these roving people, in which they prolonged their stay, as game was more or less plentiful. Traces of wild boar and kangaroo were observed in the Upper Baxter. No other large animal was known to exist. They were hunted with the bow and barbed arrow, while the war arrows were poisoned by steeping in the putrid carcase of a victim until sufficiently saturated. The district of the Baxter River contrasted strikingly with the Fly River discovered by Capt. Evans, whose banks for sixty miles swarmed with human beings. Mr. Stone's impression of the western coast was that it would prove a grave to such Europeans as should choose to reside there. This part of the country was inhabited by the Papuan race, a dark race of people, though not so dark as the Australian negro, and one of cannibal propensities. The Eastern Peninsula, on the other hand, was inhabited by the Malay race. Of this race Mr. Stone thought they had come to New Guinea from islands farther east, some of them making the change at a comparatively recent date. This race was far above the savage, both in intellectual and moral attributes. They were cultivators of the soil—each having his own plantation—and strongly opposed to the cannibalism and polygamy which obtained among their western neighbours, the Papuans. The women, too, of the Malay race were not debased as among the dark race, but mixed with the men, with whom they shared the management of public affairs. The Owen Stanley mountains ran through the centre of the country, from south to north, and the east country was on the whole favourable to cultivation, and probably possessed great mineral wealth. It accordingly offered sufficient inducement for colonisation, but colonisation, if attempted, would require to be set about with much previous consideration, owing to the peculiar situation of the peninsula and the circumstances of the people.

Mr. Kerry Nicholls read a paper *On the Islands of the Coral Sea*, which embraces that portion of the Pacific Ocean extending from the south of New Guinea, westward to the coast of Australia, southward to New Caledonia, and eastward to the New Hebrides. The New Hebrides' banks and Santa Cruz Islands, he said, constitute an almost continuous chain of fertile volcanic islands, extending for a distance of 700 miles, between the parallels of 9° 45′, and 20° 16′ south latitude, and the meridians of 165° 40′, and 170° 33′ east longitude. Espiritu Santo, the largest island of the archipelago was seventy-five miles long, and forty miles broad. The geological formation of the islands was composed of volcanic and sedimentary rocks. The chain of primary volcanic upheaval might be traced running in a general course longitudinally through the islands always in their longest direction, the axis of eruption being marked by active and quiescent volcanoes. On the north end of the island of Vanu Lava there were extensive springs of boiling water, solfataros, and fumaroles. The hot springs were of two kinds—some were permanent fountains where water was in a constant state of ebullition, others were only intermittent, and the water became heated at certain intervals, when it varied from a tepid degree of heat to boiling point. The physical features of the islands were remarkably bold, and betokened at first sight their volcanic origin. The plains, table lands, and valleys of the mountain region were, many of them, of considerable extent.

Capt. V. L. Cameron, R.N., C.B., read a paper *On his Journey through Equatorial Africa*. Capt. Cameron said that soon after entering the country from the east coast he came to a large plateau, 4,000 feet in height, encircling Lake Tanganyika, and forming the water-shed between the Congo and the streams flowing into Lake Sangora. Another table-land to the south rose to the height of 3,000 feet. The water-shed between the two basins of the Lualaba and the Congo at that part is a large, nearly level country, and during the rainy season the floods cover the ground between the two rivers, and a great portion of it might easily be made navigable. One thing he noticed in Africa was this system of water-sheds, dividing the country into portions, each having its own peculiarity, and also that in each there was a difference in the habits of the natives. Within twenty days he crossed the Nsagara Mountains and came upon a level open country where a great quantity of African corn was grown, the stalks of which rose to the height of from 20 to 24 feet. In this country no animal could live except the goat, the tsetse fly being destructive to all others. The principal geological formation was sandstone. A few marches brought him to Ugogo, an extensive plain broken by two ranges of hills, composed of loose masses of

granite piled together in the wildest confusion. The soil was sandy and sterile. Coming to the country of the Ugari he found a tribe almost identical with Unyamwesi. The principal streams of this district fall into the Mulgarazi. Unyamwesi was the commencement of the basin of the Congo. He believed that the natives of Unyamwesi were of the Malay race. They had crossed a great deal with negroes, and had lost the distinctive colour and distinctive marks of the race, but their features were much the same as the dominant races in Madagascar. Ugaro is a large plain nearly as flat as a billiard table. The people here were different from the Unyamwesians; they had not got the same features or the same tribal marks. After passing over the mountains of Komendi, which are an offshoot of the mountains round the south end of Tanganyika, they came to a fertile land, much of it laid waste by the ravages of a neighbouring tribe. All the mountains in that district were of granite. There was there a large quantity of salt and what was remarkable was that the rivers ran perfectly fresh through soil which, when the natives dug wells, gave water which was full of salt. At Ujiji the people are of a different race from those already described, as they shave their hair differently and have not the same features. Along Lake Tanganyika in some places there were enormous cliffs and hollows of rugged granite lying in loose boulders; in other places the cliffs were of red sandstone, and in others a sort of limestone and dolamite. At one place he saw exposed on the shores of the lake large masses of coal. Passing down to the south end of the lake, he found it regularly embedded in cliffs 500 to 600 feet high, with waterfalls discharging themselves down the face. Travelling along the side of the lake he came to the Lukogo, a large river more than a mile wide, but partly closed by a sort of sill on which a floating vegetation was growing, a clear passage, however, being left of about 800 yards. After proceeding some four miles up the river, Capt. Cameron's boat got jammed amongst the floating vegetation which grows to the thickness of two or three feet, and it was with difficulty the boat was extricated. The Kasongo country was next reached, the principal characteristic of which was the extraordinary trees, of which boats a fathom wide are sometimes made. Crossing the mountains of Bambara he arrived at Mamyuemba. Here he found the race entirely different from anything he had yet seen. The houses were differently built, the people were differently armed, dressed their head differently, and there was no tattooing to speak of. The villages were built in long streets thirty or forty yards wide, two or three streets being alongside each other, and a space left between the houses, which were of reddish clay with sloping thatched roof—the only houses of that description he saw in the interior of the country. All the Mamyuemba are cannibals. Journeying northwards, but still in Mamyuemba, a district was reached where iron was very plentiful, and where large forges were at work. Many of the spears and knives which they turned out looked as if finished off by a file or polished by some means, although all done by hand-forging and patient labour. The Lualaba River was next reached, which is about 1,800 yards in breadth. The southern shore is occupied by a tribe called the Wagenga, who do the whole carrying business of the river, being the only canoe proprietors, who take for pay the products of the country to the different markets. The young women make immense quantities of pottery in the mud and back water, which they exchange for fish. After referring to a country between Nywangi and Loami, where a palm oil grows in great profusion, Capt. Cameron passed through Kilemba, and reached Lake Kigongo. This lake is covered with floating vegetation, on which the people build their houses, cut a space round about them, and so transform their habitations into floating islands, so that when desirable they change the locality from one place to another. Coming to the coast he passed through one of the most magnificent countries in the world to look at, possessing a climate in which any European might live. The Portuguese had been settled in this neighbourhood for thirty years. The whole of this country was just one vast slave field. In the country there was a vast mineral wealth and an ordinary population that with education might be rendered very industrious instead of carrying on a continual warfare against each other for the purpose of obtaining slaves.

An interesting discussion followed.

Col. R. L. Playfair, H.M.'s Consul-General in Algeria, read a paper *On Travels in Tunis in the Footsteps of Bruce*. The paper gave a narrative of the Colonel's observations made in the course of a journey in Tunis over places visited by Bruce about 1763. There had been recently put into Col. Playfair's hand for publication a large number of Bruce's sketches, of which his Barbary sketches were, he said, the most interesting, forming about 120 sheets of drawings, completely illustrating the archæology of North Africa. In these circumstances, the Colonel had determined to follow Bruce in his journey, and to satisfy himself as to the present condition of those interesting ruins which were almost unknown to the modern traveller.

Mr. A. Bourden read a paper, the object of which was to show that ready access could be had to the Niger and the African interior from Sierra Leone.

The Secretary, in the absence of the author, read a paper by Lieut. W. H. Chippindall, R.E., containing *Observations on the White Nile between Gondokoro and Apuddo*. The object of the paper was to establish Lieut. Chippindall's opinion that the oft-repeated assertion that the White Nile could not be navigated higher up than Gondokoro had no warrant in fact. He was sure the White Nile was navigable all the way up to the Albert Nyanza.

A paper was read by Staff-Commander Tizzard, R.N., *On the Temperature obtained in the Atlantic Ocean, during the Cruise of H.M.S. "Challenger."* Over a great portion of the Atlantic the bottom temperature has this peculiarity—If the depth be less than 2,000 fathoms, we find the temperature at the bottom lower than that of any intermediate depth, but when the depth exceeds 2,000 fathoms, we find that the bottom temperatures are nearly the same as they are at that depth. This holds good for three-fourths of this ocean. In the remaining fourth the temperature obtained at the bottom is much lower than in the other parts, and this fourth is not at either extreme, where there is a large current of surface cold, but occupies the whole of the western portion of the South Atlantic as far north as the Equator. The results of these temperatures may be classified thus: If an imaginary line be drawn from French Guiana to the westernmost island of the Azores and from thence north on the western side of this line, the bottom temperatures at depths exceeding 2,000 fathoms are 35 degrees—that is, taking the mean of all the temperatures obtained which differ but slightly. On the eastern side of this line the bottom temperatures are 35·3 deg., and this uniform temperature appears to extend as far south as Tristan d'Acunha, as the German frigate *Gazelle* obtained similar bottom temperatures eastward of the line joining that island with Ascension to the southward of a line joining Tristan d'Acunha with the Cape of Good Hope. The bottom temperatures are decidedly colder between the eastern coast of South America and a line joining Tristan d'Acunha and Ascension Island; and from the Equator to the southward the bottom temperatures were invariably colder than at any intermediate depth. These temperatures varied from 31 deg. to 33 deg. 5 sec., that is when the depth exceeds 2,000 fathoms, and temperatures of less than 33 deg. were found as far north as the Equator, while a few miles northward this bottom temperature was 35 deg. It therefore appears that in the western portion of the South Atlantic the highest bottom temperature is less than the lowest obtained elsewhere in this ocean, excepting where the very low result of 29 was found by the *Porcupine* in 1869 between the Faroe Isles and the north extreme of Scotland. The question thus arises as to the causes which confines this cold water to the bottom portion of the western half of the South Atlantic. The examination of the soundings which had been taken in this ocean, combined with the results of their temperature, leads to the conclusion that there is a series of ridges dividing its bed into two basins, one of which occupies the whole of the western portion of the North Atlantic, while the other extends the whole of the length of the ocean on its eastern side, and that the cold water in the western portion of the South Atlantic is owing to there being no obstruction between the bed of this portion of the ocean and the bed of the Antarctic basin, and from the results of the serial temperatures' soundings it would appear that these ridges cannot exceed 1,950 or 2,000 fathoms in depth. To ascertain the thermal condition of the Atlantic (from the surface to the bottom), serial temperatures were obtained in the *Challenger* at 150 positions, observations having been made at each 100 fathoms to 1,500 fathoms in depth, and frequently at, say ten fathoms to 200 fathoms in depth, at each of these positions. An examination of these temperatures shows that between the parallels of 40 deg. N. and 40 deg. S. there is a much larger amount of warm water in the North than in the South Atlantic, and that in the equatorial regions the isotherm of 60 deg. is much nearer the surface than in the temperate zones, but that the isotherms below 60 deg. are at nearly as great a depth at the Equator as in any part of the South Atlantic, especially at the isotherm of 40 deg.,

and that between the parallel of 30 deg. and 40 deg. N. latitude, the isotherm of 60 deg. occupies a depth of 300 fathoms, over an area of 1,200,000 square miles, while the average depth of this isotherm between the parallels of 30 deg. and 40 deg. S. latitude is 160 fathoms; also that the isotherm of 40 deg. which is at an average depth of 800 fathoms across the North Atlantic, between the parallels of 30 deg. and 40 deg. N. latitude, occupies only half that depth in any part of the South Atlantic. This phenomenon may be explained in the following manner:—The power of the sun indirectly heating the water below the surface appears not to extend below 100 fathoms even in the tropics, and this power decreases as the higher latitudes are reached, until a position is attained where the temperature is that of the freezing-point of salt water. As salt water at its temperature of congelation is denser than at any higher temperature, its weight would cause it to sink, and it would in time, did no other cause intervene, occupy the whole of the space in the ocean not influenced by the sun's heat. But in considering the effect of the heat imparted to the surfaces we have also to consider the effect of evaporation and precipitation. In the equatorial regions evaporation is rapid, so that the surface film would become cleared through increased salinity were it not for the increased temperature and large precipitation, as well as to its being transported by the friction of the trade winds and earth's motion to the westward. This surface film, constantly moving westward in the equatorial regions, meets in the Atlantic with an obstructing point of the South American continent, which directs it to the northward, so that the greater part of the water directly heated by the sun's rays in the tropical regions is forced into the North Atlantic. As the salinity of this water is greater than that of the subjacent layers, and its increased temperature only renders it less denser, directly a portion of this temperature escapes in the colder regions of the temperate zone, the surface film sinks and imparts heat to the water beneath. Consequently, the isotherms will be found at greater depths where the heated surface films are constantly descending than when, owing to their being less denser than the subjacent layers, they remain on the surface.

Mr. J. Murray stated some results of his observations on board the *Challenger—On the Geological Distribution of Oceanic Deposits*. These deposits were stated to be of three classes—first, those which were found all round the continents and islands existing over the world, without any exception, but which varied according to the places where they were found; secondly, those found at from 200 to 300 miles from the land, consisting of shell and lime deposits, and covering most of the bed of the ocean; thirdly, those existing at other depths, and which were of silicious character. The observations showed that a curious relation existed between the nature of the deposits and the depth of the water. It was also pointed out that in the neighbourhood of volcanic islands, and in no other places, were found large deposits of manganese, coating the shells and other things brought up from the bottom.

Mr. Buchanan submitted a communication of observations of the *Challenger*, bearing upon *The Specific Gravity of the Surface Water of the Ocean*. He also explained the principles on which he constructed a new deep-sea thermometer with which his observations were made.

Professor Porter read a paper *On some Points of Interest in the Physical Conformation and Antiquities of the Jordan Valley*. The general geological structure of the valley was, he said, of lime, and of the same age as the basin of the Sea of Galilee, and its surface was flat. The breadth varied from three to ten miles, extending a little towards the east, and from the nature of its thick alluvial covering, it was of more recent formation than of the mountains, the valley having been at one time apparently a lake, of which the soil was the deposit. The river Jordan as it at present existed, could have had nothing to do with the formation of the valley itself. He recommended to the notice of men of science that geological remains on the site of Sodom and Gomorrah pointed to an explosion of bitumen much later than the ordinary geological formation, and probably within the historic period.

Signor G. E. Cerruti read a paper *On his Recent Explorations in N. W. New Guinea*. After several visits to the islands and part of the mainland on the north, he was in 1869 sent out by Count Menabrea for the purpose of making investigations preliminary to the formation in New Guinea of a penal settlement. He secured at the same time means for turning his expedition to profit geographically. He believed that a great part of the region from the Xulla Islands to New Guinea, and perhaps more to the north, had been subject to very important volcanic action in an epoch not very far distant, and one could see the work now going on—the western coast showing gradual subsidence. But whatever the origin of the islands, they were now covered with a vegetation which he had not found equalled in luxuriance in any part of the world. He urged in strong terms the colonisation of New Guinea.

This Section was brought to a premature close on Tuesday the 12th from want of an audience. The meetings were held in the Queen's Rooms, at a considerable distance from the University, which no doubt to a great extent accounts for the poor attendance.

SECTION F.—Economic Science and Statistics.

Dr. William Jack read a paper *On the Results of Five Years Compulsory Education*. After entering into considerable details as to the working of the system, he concluded that he had established the following points:—1. That the need of the country for compulsory education was a crying need in 1870. 2. That the success of the experiment which has now been tried in Scotland, and in nearly half of England, justifies the very modest advances that have been made by the Government in the bill of the present year. 3. That compulsion has been carried out in one great city with perfect efficiency, and with a very trifling amount of legal process. 4. That there is no agency short of compulsion which can bring Ireland on a level in popular education, with her sister countries. A very interesting discussion followed the reading of this paper.

Mr. J. Heywood, F.R.S., read a paper *On the Memorial of Eminent Scientific Gentlemen in favour of a Permanent Scientfic Museum*. He advocated the placing on a permanent basis an institution similar to the Loan Scientific Institution now open at South Kensington.

The Rev. Dr. M'Cann then read a paper *On the Organisation of Original Research*, in which he advocated an exceedingly elaborate system for carrying out the object in view.

After some discussion in which Dr. Jack, Professor Hennessy, of Dublin, and others took part, Mr. Heywood submitted the following resolution—"That this Section approve of the maintenance of a scientific museum in London, containing scientific apparatus, appliances, and chemical products."

Sir George Campbell, in summing up the discussion, said he should support this motion, and he also agreed with Dr. M'Cann that there should be a national system of scientific education.

The motion was unanimously passed.

An important discussion took place in this Section *On the Civilisation of South-Eastern Africa*, caused by the reading of a paper on the subject by Mr. Stevenson.

SECTION G.—Mechanical Science.

This Section met under the presidency of Mr. Charles W. Merrifield, F.R.S., who in his address spoke of our shortcomings in those subjects of instruction which are the necessary preludes to mechanical science. He urged the importance of physical science as that which had given us command over the material powers of nature, and which alone could enable us to keep pace with other nations in industrial competition, and to maintain the health of crowded populations. With their populations, which had more to fear from war and famine than from want of elbow-room, political and historical knowledge in the governing class was more important than exact knowledge in the administrative class; but as the population thickened, the latter assumed more importance; and while he did not think political wisdom would ever lose its value, he thought it only a part of such wisdom to recognise that in such communities as ours the spread of natural science was of more immediate urgency than any other secondary study. One of the obstacles to the spread of science and to our national prosperity he took to be the undue preference given to literary over natural knowledge, and in particular the sacrifice of mathematics to classical study in the secondary schools. Apart from the general fault of giving too low a place to mathematical teaching, a great fault was our not paying sufficient attention and sufficiently early attention to mechanical and geometrical drawing. He concurred with a remark of Professor Fleeming Jenkin that descriptive geometry was not what was wanted. A much more important exercise of geometry, and one more immediately useful, was the geometrical representation of arith-

metic, such as was seen in diagrams of thrust, pressure, speed, and so forth. But this would take care of itself provided linear drawing were taught sufficiently early. Passing on to discuss certain points connected with the crowding of the population, he remarked that the real problem of civilization had been to render life tolerable in large aggregations, and that this problem was yet only partially solved. Among the difficulties of town life he reckoned—(1) the insufficient supply of fresh air; (2) the mere proximity of persons facilitating the spread of contagious or infectious disease; (3) the getting rid of excreta or waste products; (4) a wholesome water supply to be provided and kept pure.

Mr. Baldwin Latham read a paper *On Hydro-Geological Surveys*, in their bearing on health. He dwelt on the importance of ascertaining the sub-water course, and making certain that the well was on a higher level, so that it could not be contaminated by cesspools or other pollutions. These surveys showed the absolute necessity of sewers being made water-tight.

Mr. W. J. Millar read a paper *On the Strength and Fracture of Cast-Iron*. The author described the results obtained in testing cast-iron bars 36 inches span, 2 inches deep, and 1 inch broad. The bars usually broke with straight fractures, but occasionally curved fractures were observed. The average breaking strength of 29 bars showing straight fractures was 3584 lbs., the average strength of 25 bars showing curved fractures was 3551 lbs. Some results of "set" and deflection were given, showing that for successive applications of the same load, 2800 lbs., there was a decrease of set. The principal object aimed at by the author of the paper was to show the relation existing between form and position of fracture, straight fractures taking place at or close to centre of span, and curved fractures occurring at points more or less removed from centre of span.

Sir William Thomson read a paper *On Naval Signalling*, in which he advocated the use on board ship of the fog signalling system instead of the flag system now in use. His method is simply this—to signal according to the Morse telegraphic code by means of two sounds of slightly different pitch. For the long signals he would take a grave note, and for the short signal a less grave note, or what he might call an acute and a grave note for the dot and the dash. Sir William Thomson then gave several signals to show the efficacy of the plan he proposed, and he maintained that the shortness of the time required to make flag signals was far less than could be attained by the phonetic method. Long before the signal flags could be hoisted, the order would be given and read by every ship, and repeated by the different ships in order, back to the admiral. Two sounds of different pitch made in rapid succession was all that was necessary, and to accomplish this all that was required was two steam whistles, each with a different note.

Many other papers of great value were read both in Sections F and G, but as they were mainly technical, or very special, our space prevents us referring to them in detail.

THE CHALLENGER EXPEDITION[1]

THE task which I have undertaken this evening—to give a general sketch, however slight, of the work and results of the *Challenger* expedition in the space of a single lecture—is by no means an easy one, for two reasons. The various lines of inquiry bear on so many different subjects, and these dovetail into one another in such a complicated manner, that it would take many hours to explain them even in the most superficial way. The other reason is that the observations which were made during the *Challenger* expedition have only as yet been very imperfectly examined, and only half digested, owing to want of time, and the great collections in natural history which were brought home in the ship have been only glanced at, and it is therefore scarcely safe for me to use either the observations or the collections as the bases of generalisation. I must therefore this evening, in this address, only be regarded as giving a most elementary idea of the objects of the expedition and its results, and what I say must be regarded as preliminary, and subject to further reconsideration. Still, some new and remarkable facts and phenomena which have hitherto been unknown, or only vaguely guessed at, are sufficiently definite, and I will devote the short time at my disposal to the consideration of one or two of these. The superficial area of this world of ours is about 197,000,000 of square miles, and of these about 140,000,000 are covered by the blue sea at an average depth of 2,500 fathoms—about 15,000 feet. This vast region under the sea has not until comparatively recently excited much curiosity. It seemed to be practically inaccessible, and certain hasty and incorrect assumptions in regard to some of its conditions had reduced it to a barren uniformity and divested it of any interest. The laying of deep-sea cables for the purposes of ocean telegraphy, by bringing to light certain phenomena which threw a doubt upon previous conclusions, stimulated inquiry, and gave rise to new speculation; and the systematic scientific exploration of the depths of the sea by several special exploring expeditions put our knowledge upon a totally different footing. We now know that the sea covers a vast region which is to a certain degree comparable with the land—a region which has its hills, valleys, and great undulating plains; that it has its various soils—widely different materials laid down and accumulated in different places; that it has its climates, whatever the very exceptional conditions of those climates may be; and that it has its special races of inhabitants which depend, like the inhabitants of the rest of the world, upon the conditions of climate and on the nature of the soil for their distribution.

The *Challenger* expedition was despatched on a very special errand—to investigate the physical and biological conditions of the great ocean basins. And, under this general heading, certain more minute instructions indicated the particular questions, physical and biological, which were specially to engage our attention. We were instructed throughout our long course, which extended round the world and traversed the Atlantic and Pacific Oceans and the Southern Sea so far south as it was possible to go without running the risk of being entangled for a winter in the ice—a contingency for which we were not prepared—to select certain stations at convenient distances, and at each of these to determine certain points. We were to determine, in the first place, the exact position of the station; then, with the best appliances at our disposal, we were to determine the precise depth; we were to bring up by means of the sounding apparatus a certain amount of the material of the bottom for microscopical examination and for chemical analysis; we were to bring up a specimen of the water from the bottom for analysis and physical examination; we were to determine the bottom temperature with accuracy; and we were to determine the temperature of the sea at different levels from the surface to the bottom; we were to get specimens, if possible, of the sea water from various depths. Lastly, we were to endeavour, by the use of the trawl or dredge, or any other instrument which might be suitable, at each station to procure a fair sample of the creatures which inhabit the bottom, and in this way to get, if possible, a general idea of the fauna inhabiting the depths of the sea. The instructions of those in charge of the scientific departments in the *Challenger*, both naval and civilian, did not, however, by any means end here. The officers had been selected in order that they might study by the light of their own previous experience the bearings of those various data upon one another, and this was a very serious addition to the work of the expedition. It was found necessary, in order that this might be carried out to its fullest extent, that the instructions given by the Admiralty should be comparatively flexible, and that the details of the working of the ship should be left to a certain extent to the captain of the ship and to the director of the scientific staff, so as to enable them to deviate from any definite line or course when it became desirable for any purpose that they should do so. I have only to add that the equipment of the vessel was such as to leave very little to be wished for, and that the liberal arrangements of the Admiralty, which were admirably carried out by the Hydrographic Department, worked in the most satisfactory way. The *Challenger* left Sheerness on December 17, 1872. She crossed the Atlantic four times during the year 1873, and along a course of nearly 20,000 miles she established 150 observing stations, at each of which, with few exceptions, all the required observations were made. In 1874 she went southwards from the Cape of Good Hope, spending nearly a month among the southern ice, and dipping within the Antarctic Circle, as far as she could with safety, considering the lateness of the season and her unprotected condition. She then traversed the seas of Australia and New Zealand, and made some most interesting observations among the islands of the Malay Archipelago. She arrived at Hong Kong on November 10, having run a course in the year 1874 of upwards of 17,000 miles, along which sixty-six observing stations had been established. In 1875 she traversed the Pacific, with a course of about 20,000 miles and 100 stations; and in the early part of the present year she crossed the Atlantic for the fifth time, filling up here and there blanks in her former

[1] Report of Address given at the Glasgow Meeting of the British Association, September 11, by Sir C. Wyville Thomson. Revised by the Author.